

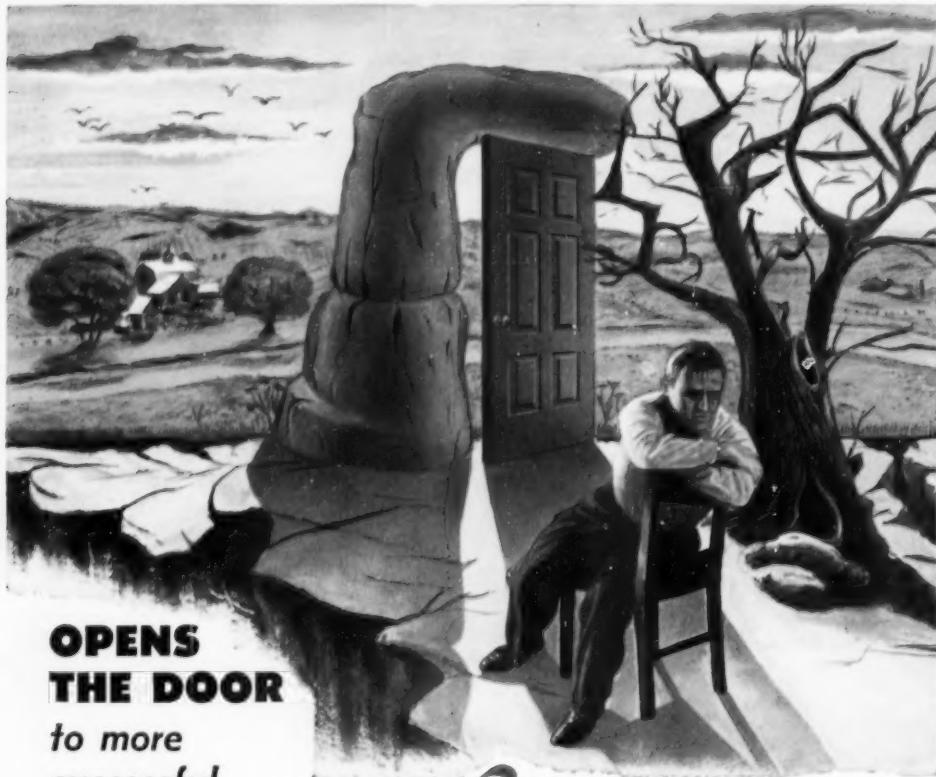
# THE AMERICAN JOURNAL of PSYCHIATRY

**VOLUME 113**  
**NUMBER 5**  
**NOV. 1956**

*Official Organ of*  
**THE AMERICAN**  
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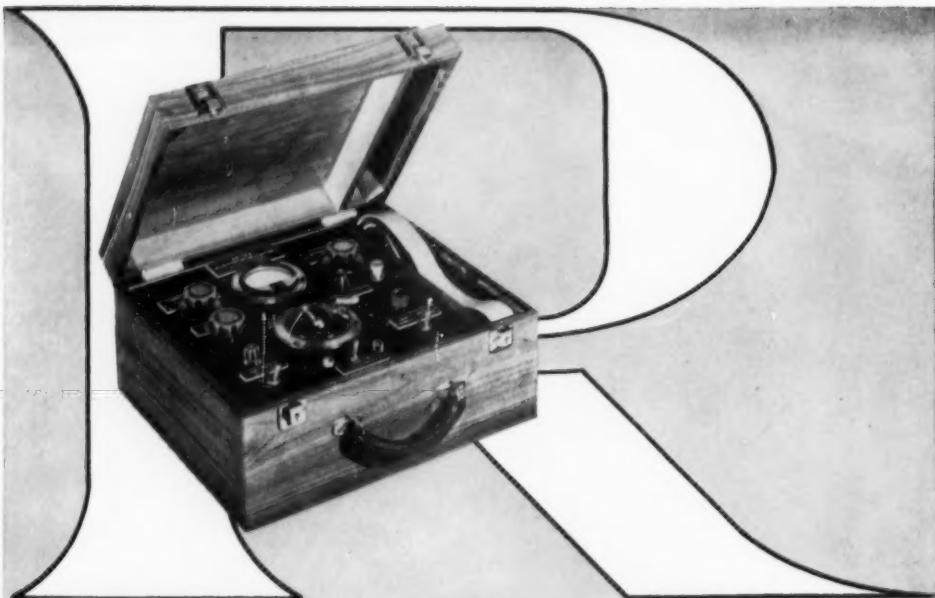
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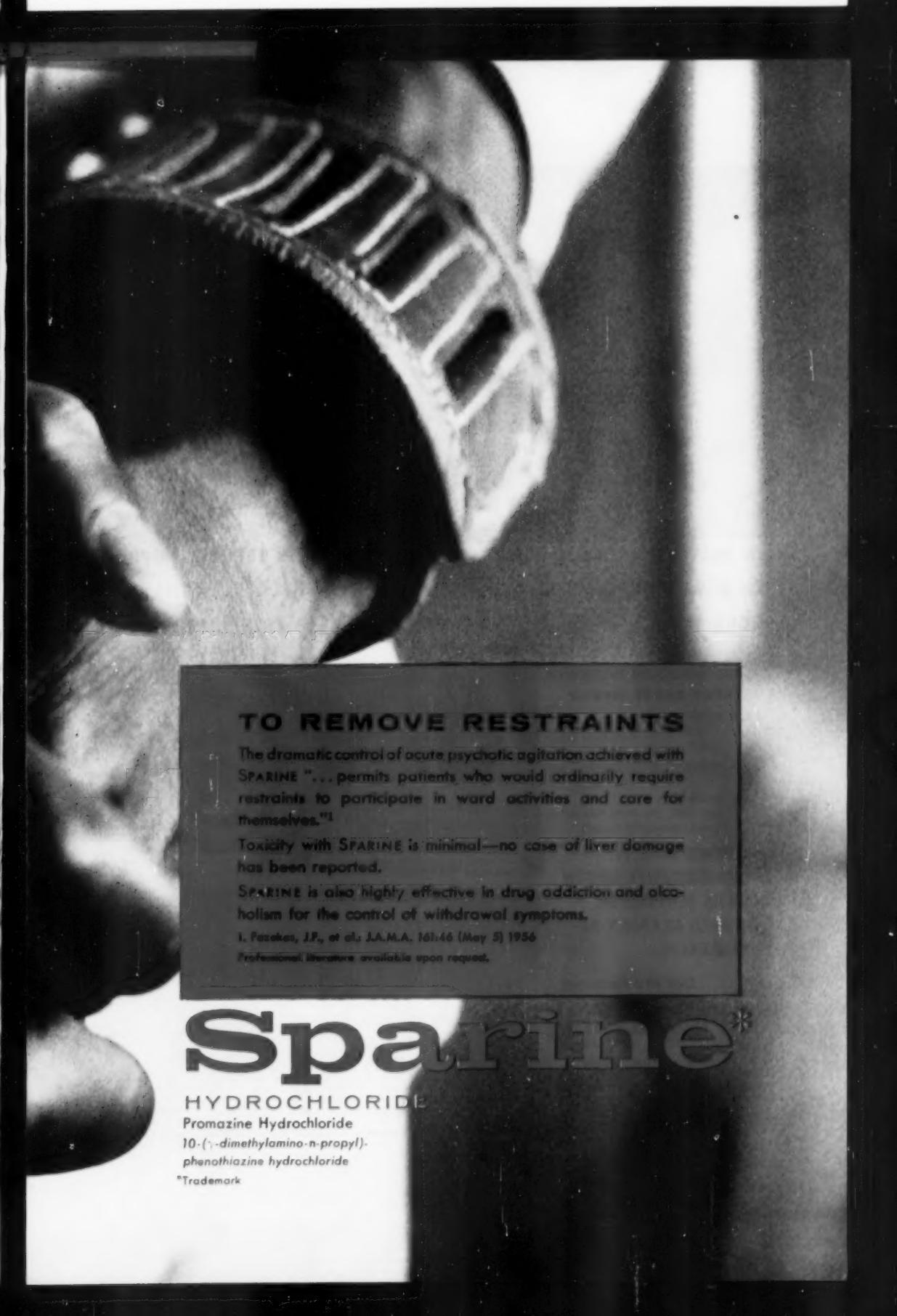
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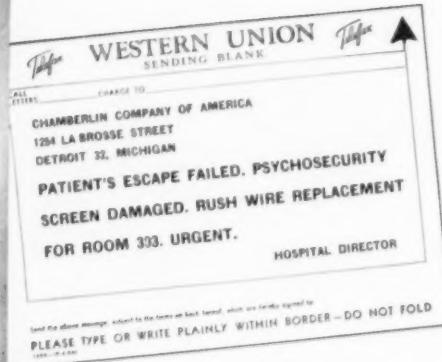
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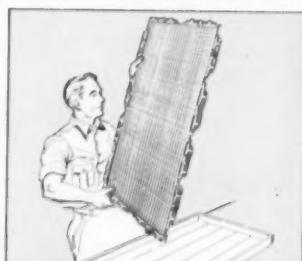
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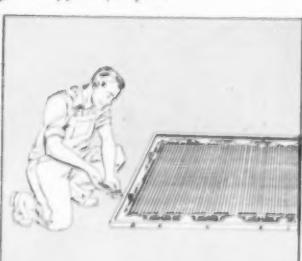
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1. Katz, E.M., and Kowaliczko, Z.: Internat. Rec. Med. 169:596 (Sept.) 1956. 2. Barrabee, P.; Wingate, J. H.; Phillips, B. D., and Greenblatt, M.: Postgrad. Med. 19:485 (May) 1956.



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1. Branceland, F. J., and Jackson, M. B., in Fishbein, M.: 1956 Medical Progress, New York, McGraw-Hill Book Company, Inc., 1956, p. 258-60.

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1. Proctor, R. C., and Odland, T.: *Dis. Nerv. Syst.* 17:25, 1956. 2. Fabing, H. D.: *Neurology* 11:3, 1955. 3. Branceland, F.; Rudy, H., and Hinrichs, H. E.: *Am. J. Psychiat.* 112:343, 1955. 4. Hinrichs, F., and Hinrichs, H. E.: *Science* 121:198, 1955. 5. Ferguson, J. T.: Frenquel: Use of a new pharmacologic agent in chronic schizophrenia, presented before the Am. Psychiat. Assoc., Chicago, Ill. April 30, 1956. (To be published)

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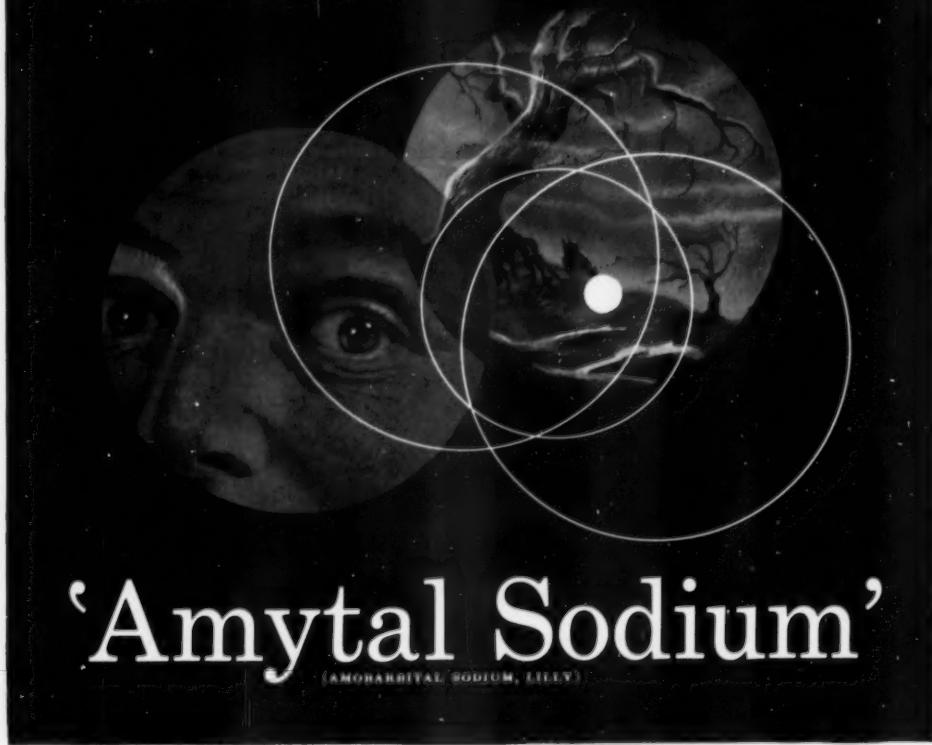
*senile dissociation states*

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**INTRODUCTION OF DR. PERCIVAL BAILEY, ACADEMIC LECTURER  
THE AMERICAN PSYCHIATRIC ASSOCIATION 112TH ANNUAL MEETING****FRANCIS J. GERTY, M. D., CHICAGO, ILL.**

My acquaintance with the man who is to deliver the 1956 Academic Lecture of The American Psychiatric Association began over 30 years ago. For the last 15 of these years, he has been a colleague and a valued friend on the faculty of our medical college at the University of Illinois. When he comes before you, you see the assistant, the associate, and a major scientific successor of Harvey Cushing. His classification of the brain tumors in Cushing's collection on an embryologic basis is our standard classification of these tumors. We cannot overestimate the impetus given to Cushing's work when he became Cushing's first postwar assistant. He studied the case histories of patients with brain tumors and was the first to point out the very high incidence of emotional disturbance in tumors of the third ventricle which impinged upon the hypothalamic area.

In 1922, when still a very young man, he was chosen for the high honor of reading a paper in French for Cushing at the *Troisième Reunione Neurologique Internationale* in Paris, with Babinski and Monrad-Krohn in attendance. In 1939 Cushing, at his birthday dinner, had this to say early in the course of his response to the many addresses of tribute that were made: "So you see I've spent my neurosurgical life leaning heavily on others, on Percival Bailey and his pupil, Louise Eisenhardt," and then went on to name a long series of his assistants and associates.

In 1928 he became neurosurgeon and Professor of Neurology at the University of Chicago. When he became a member of the faculty at the University of Illinois in 1939, it was with the title of Distinguished Professor of Neurology and Neurological Surgery. Here, at the Neuropsychiatric Institute, working alone and also with Oldberg, McCulloch, Bucy, Gibbs and others, he did much valuable work upon the neurophysiological and neuropathological bases of behavior and its disturbances. To the whole medical and

scientific world he is known and honored as a great neuropathologist, clinical neurologist, and neurological surgeon, and few living neurologists could justly advance a better claim to lasting fame.

Nevertheless, were I to cease at this point, your minds might justly question the wisdom of inviting him to give this Academic Lecture. I might well turn to him and say: *Don Quixote, valiantly charge the heights before you where sit the psychiatric windmills. They can unhorse a man!*

But the picture is not complete. Our speaker has had something to do with psychiatry, too. In 1920, he served as a resident in psychiatry in the Cook County Psychopathic Hospital. During the 2 years he spent in Paris, he was a close attendant upon Pierre Janet's clinical work and lectures, and, on occasion, can be persuaded to give a talk on his experiences under Janet's tutelage. He is a charter member of the Illinois Psychiatric Society. He has always collaborated fully and closely with the work of the psychiatric division of the Illinois Neuropsychiatric Institute. When he was appointed Director of the State Psychopathic Laboratory of the Department of Public Welfare of Illinois in 1951, he set about vigorously to reorganize its training and research program.

As Chairman of the Psychiatric Advisory Council of the Department of Public Welfare, he has worked closely with the 7 psychiatrists appointed to that body by the Governor. With them, he has initiated many undertakings important for mental health in the State of Illinois. Among these are the making of grants for psychiatric research from the state mental health funds; the establishment of a Psychiatric Institute upon which construction will soon begin; and probably most important of all, the creation of a Psychiatric Training and Research Authority which will provide \$1,000,000 a year for grants for continuing psychiatric research and training programs in the State of Illinois. The Psychiatric Institute and the Authority

have become facts through statutory establishment.

Perchance, and this I do not certainly know, though I know his general trend of mind, his remarks to us this day may be somewhat critical of some aspects of psychiatry. If so, remember that the critic has also been a performer. It is not again the case of English bards and Scotch reviewers. He has not scorned to work and help us in our field. Should he speak of lobotomy, observe that he is a skilled brain surgeon. Should it be the tranquilizing drugs, bear in mind that he has encouraged research with them at the Galesburg State Research Hospital. If he chooses to speak of the training of psychiatrists, it is to be noted that he has lived in close touch with training programs as a resident physician and teacher, and has borne

partial responsibility for the conduct of some of them. It is possible that he will ignore neither shock treatment nor psychoanalysis. He has worked with Meduna, originator of convulsive therapy of the psychoses, and has spent many long hours with psychoanalysts, such as Alexander, Grinker, and Carmichael, though these hours have been to plan incorporation of analytic training in residency programs and to design hospital facilities and promote research, not for personal analysis.

If his name were not printed in the program and I had not mentioned it earlier, it would have ceased to be a mystery after my first few sentences. I introduce to you an Honorary Fellow of this Association who will deliver the Academic Lecture—Dr. Percival Bailey.

## THE ACADEMIC LECTURE

### THE GREAT PSYCHIATRIC REVOLUTION<sup>1</sup>

PERCIVAL BAILEY, M. D.<sup>2</sup> CHICAGO, ILL.

Be his stuff begged, borrowed, or stolen, or of his own domestic manufacture, he is an uncommon man. Swear he is a humbug—then he is no common humbug. . . . The truth is that we are all sons, grandsons, or nephews or greatnephews of those who go before us. No man is his own sire.

HERMAN MELVILLE

In my youth I was greatly interested in psychiatry and thought seriously of entering the field as my life's work. For reasons which are here irrelevant, I went instead into brain surgery. Now, more than 30 years later, for reasons equally irrelevant on this occasion, I find myself abandoning surgery and preoccupied with the problems of psychiatry. In my long absence from the field, I had heard that a great revolution had taken place (169), so I was all eagerness to plunge into the new era. Since I still remained a physician, I never doubted that the poor patients, who had aroused my interest in the first place, were now much better off. After 3 years of reading, listening, and observing, I find myself in a state of grievous bewilderment<sup>3</sup>. I had forgotten that revolutions bring change but not necessarily progress. Today I propose to come to grips with many of the recent developments in psychiatry, both physical and psychological, not forgetting, as Oberndorf (129) says, that "psychoanalysis began to revolutionize psychiatry fifty years ago."

Having come from the surgical clinic I naturally began first to assess the results of the surgical treatment of psychiatric disorders. I knew, of course, that many psychiatrists (14) had grave doubts of the advisability of these interventions, but it seemed to me that their objections rested mainly on theoretical grounds, derived either from theological considerations or perhaps from vague unconscious memories of the teaching of Franz

Josef Gall. I had done a few such operations myself, without scruples, feeling that there was no more reason to hesitate to remove part of the brain than part of the thyroid gland, if the patient stood to gain thereby. Nevertheless, the reaction of one of my own patients had disturbed me. After a lobotomy she turned to her husband and launched into a tirade so filthy that it would have made a sailor blush, then looked at me in glee and remarked, "I have been wanting to do that for 30 years." Here was a way of bringing repressed material into the open with a vengeance. There was also that other patient who, after a frontal lobectomy, brought into his company, for which he was a salesman, so many orders that the company had to make a rapid merger with another in order to cope with them. Then the company made him executive vice-president; within a year the company was bankrupt. Such experiences gave me pause; it may be true that the troubling of the intellect is difficult to detect by the usual tests of the psychologist (82) but it seems that Franz Josef Gall was not entirely wrong.

Now, after the wave of psychosurgery has swept across the country, leaving hecatombs of mutilated frontal lobes behind it, I read many accounts of the results and am frankly appalled. Abusive and obscene language, disability in long-time planning, uninhibited sexual drive, obnoxious mannerisms, stealing, suggestibility, laughing spells, convulsions, and other untoward symptoms are frequent (0, 128) and discouragingly persistent in spite of intensive reeducation. The most favorable result seems to be that the disturbed patients, after lobotomy, are easier to manage in the hospital. But, as Wiener (162) remarked, they would be still less trouble if taken to a gas

<sup>1</sup> Read at the 112th annual meeting of The American Psychiatric Association, Chicago, Ill., April 30-May 4, 1956.

<sup>2</sup> Director, Illinois State Psychopathic Institute.

<sup>3</sup> Agur ben Jakeh: "Surely I am more brutish than any man and have not the understanding of a man. I neither learned wisdom nor have the knowledge of the holy" (Proverbs 30:2, 3).

chamber as the Nazis recommended. Grave doubts have arisen(115) concerning the justifiability of using this substitute for better administration, more personnel and larger budgets. So that classical lobotomy has been forbidden in the Soviet Union(147), severely criticized by the Pope(136) and is rapidly disappearing from our practice. It relieves anxiety but at too heavy a cost.

Does this mean that all forms of surgical intervention on psychiatric patients must be banned? As soon as untoward symptoms began to appear after lobotomy, suggestions were made that perhaps they could be avoided by removing only parts of the frontal lobe, and still obtain the desirable transformations of character. But which parts? It was quite natural to think of the ubiquitous and supposedly precise chart of Brodmann, as intriguing as the phrenological chart of which it was a lineal descendant. It rested on the belief, unconscious or avowed, that a difference in structure means a difference in function. It was necessary, therefore, to outline areas in the cerebral cortex which had distinctive structure and then find out by experiment or observation what the functions of these areas were. The derivation of this program from Franz Josef Gall is obvious and it was so plausible that the surgeons, including myself, with their customary enterprise, began enthusiastically to put it to the test. Everyone knew of the striate cortex and its relation to vision, and of the motor cortex and its relation to movement; it was easy to recognize them under the microscope. When, however, I began to try to check the accuracy of my extirpations from the frontal pole I found myself, in spite of years of experience with the microscope, unable to recognize the areas of Brodmann. When I went to the literature for help I found to my dismay that Brodmann(23) had published only a chart; the data on which it rested were unavailable. Of course, Economo had produced what Lorente de Nò called an unsystematic elaboration of Brodmann but now my suspicion was aroused. Therefore, I began a study of cytoarchitectonics which lasted for years and culminated in the publication, with Gerhardt von Bonin(9), of a monograph on the human cerebral cortex in which we reached the conclusion that the subdivisions which Brod-

mann had made in the frontal eulamine cortex were inconstant and unimportant. Brodmann himself had said they were *etwas willkürlich*, an understatement worthy of an Englishman. We rejected still more vigorously the infinite parcellations of the school of Oskar Vogt. Meanwhile, I had ploughed through all of the voluminous and complicated writings of the Vogt school—days and weeks and months of laborious cross-checking—suffering constantly from the symptom called by the French psychiatrists the *phénomène du déjà vu* as one meticulous description after another proved on analysis to be merely a paraphrase of the preceding. I am persuaded that I am the only man living who has performed that onerous task, and maintain that only three other men have cost me so much fruitless travail. They are, as we shall see, Franz Josef Gall, Thomas Aquinas, and Sigmund Freud. As a result of all this travail I have turned my attention away from the cortex to the subcortical structures and now pin my hopes on Clarke's stereotaxic instrument. At any rate, the great neurosurgical revolution has proven abortive; it has not emptied our state hospitals. Whether similar destructions in the deeper regions of the brain will prove any more helpful remains to be seen. Remembering the results of the great epidemic of lethargic encephalitis, now largely forgotten by the psychiatric theorizers but of which Jelliffe (95) once said: "In the monumental strides made by neuropsychiatry during the past ten years no single advance has approached in importance that made through the study of epidemic encephalitis," we may be sure that deep lesions of the brainstem will produce alterations in behavior. Whether these will be helpful to the patients may be doubted when one thinks of the findings of Klüver and Bucy(105) in monkeys and the unfortunate results of those(153) who have rashly reproduced them in human beings.

In the development of his method of treating psychiatric disorders by frontal lobotomy, Egas Moniz(124) was prodded by the observations of Jacobsen and Fulton on the results of experimental lesions of the frontal lobes in chimpanzees. Other methods of treatment have developed from the observation of natural experiments in man. So it

was noted that psychiatric symptoms were often relieved by intercurrent infection and this led to the treatment of syphilis of the brain with malaria. It was also known that shocks of various kinds might bring relief. In 1787 a German physician, Scheidemantel (145), wrote a book entitled, *Die Leidenschaften als Heilmittel betrachtet*. This is, therefore, no new idea but, after the work of Sakel, Meduna, and Cerletti, the modern vogue of shock therapy spread like an epidemic across the face of the globe. At first the reports were very enthusiastic. In reading them I suffer again from the *phénomène du déjà vu* which, I begin to suspect, will be one of the *Leitmotive* of this lecture. Much the same panegyrics attended the spread of the shock gospel as had the spread of lobotomy and, I might add, as attended, in a previous generation, the spread of phrenology.<sup>1</sup>

However that may be, criticisms (14) of shock therapy were not long in arising, just as happened with lobotomy. These did not arise from the occasional physical accidents

such as broken backs. They arose from more theoretical considerations. In the first place, no one knew how the method worked, when it did. In the case of general paresis it was possible to demonstrate that the malarial infection destroyed the spirochetes which caused the disease. But the diseases treated by shock therapy, of whatever variety, had no known etiology. It was, therefore, a blind empirical treatment. Also, it was too liable to abuse; its use saves much time which would otherwise have to be spent in interviews and therapeutic sessions.

There is also a punitive aspect (15) which cannot be denied. It brings memories of older psychiatrists who intentionally punished their patients as a means of therapy.<sup>2</sup> Aulus Cornelius Celsus said that they must be chastised by hunger, chains, and fetters. Patients dread electric shock, if they have not seen it administered to others, less than they do metrazol shock but there is, nevertheless, an unconscious memory (15) which becomes apparent in panic reactions at the sight of certain apparatus. Moreover, it does not produce permanent therapeutic results. Long experience seems to have reduced the use of electric shock to melancholia, especially the involutional type, in which it sometimes cuts short the attack in spectacular fashion.

Insulin therapy is less dramatic and brutal but its effects are also transitory in schizophrenics and there is a temptation to use it, as is the case with lobotomy, as another substitute for better administration, more personnel and larger budgets. Moreover, this treatment sometimes leaves organic syndromes more serious than broken backs, such as loss of memory translating diffuse destruction of brain tissue, or impulsive and violent behavior. All of these considerations again bring echoes from the past. Esquirol (43) re-

<sup>1</sup> It will be recalled (36) how Amariah Brigham, one of the founders of this Association, was so impressed by that movement that he had lectures on the subject given to the staff in his institution and reported that they listened with rapt attention. Isaac Ray (133) translated the great work of Gall and Spurzheim and remarked (36) that "no story-book was ever devoured with such abandon of every thought as Gall's great work *Sur les Fonctions*." Horace Mann (36) "regarded phrenology as the greatest discovery of the ages and built all his theories of mental and moral improvement upon the ideas which it furnished him. . . . Upon the physiological studies of Gall had been erected a mighty superstructure variously termed a social science, a universal philosophy, a guide to life itself." It invaded the fields of penology, health, literature (Poe, Melville, Whitman), science (Morton), medicine, psychiatry, religion, and sociology. How could I avoid the *phénomène du déjà vu* when I read (129) recently: "By founding and developing psychoanalysis Sigmund Freud inaugurated a movement which has penetrated and enriched so many fields of thought and endeavor that it would be difficult to list them all. Some of those fields are, besides psychology and psychiatry, sociology<sup>2</sup> anthropology (142), mythology, the various fine arts, religion, philosophy, education." In spite of Franz Josef Gall, who had certain scientific scruples and wrote protests against such *wilde Phrenologie*, studies were published of the characters of prominent men long since dead as well as of those living. There is still extant a phrenological chart of the head of G. Stanley Hall, who brought Freud to this country, if anyone is interested in consulting it.

<sup>2</sup> Sasz (143): "Does it not follow, then, that when we propose to apply psychiatry [read psychoanalysis] to social change the relevance of the alleged theoretical foundations for change rests on extremely shaky ground?"

<sup>3</sup> Benjamin Rush (140) recommended, "If all these modes of punishment should fail in their intended effects, it will be proper to resort to the fear of death. By the proper application of these mild and terrifying modes of punishment chains will seldom, and the whip never, be required to govern mad people."

marked that "Van Helmont advises that one leave the patient under water until he loses the use of his senses."<sup>1</sup> It would seem that the renaissance of shock therapy is not the answer to our problems.

Lately, the crowd has rushed to a new series of drugs. Rauwolfia and chlorpromazine are the watchwords of the hour. They are driving lobotomy and shock therapy from our hospitals. Enthusiastic reports appear weekly.<sup>2</sup> But there have been previous similar enthusiasms concerning other drugs. A good example is that of William Wright (168) for sodium bromide which he praised in essentially the same words a quarter of a century ago. It seems then that neither the bromides, Rauwolfia, chlorpromazine, nor any other drug will solve our problems. They may calm the agitated patient long enough to make him more accessible to other forms of therapy, or long enough to let nature take her course, and so make him easier to care for while he remains in the service, but they have also deleterious effects and cannot be

<sup>1</sup> Pinel(135) remarked thereupon: "The bizarre idea of Vanhelmont was to destroy, to their very roots, the ideas of the patients which, according to him, could be accomplished only by obliterating, one might say, these ideas by a state near to death. But one should blush to insist on this medical delirium, perhaps still worse than that of the patient whose troubled reason one wishes to restore."

<sup>2</sup> Noce, Williams, and Rapaport(128): "From our experience to date we believe that about 80% of psychiatric patients show improvement that is attributable to the alkaloid. Depressed patients become alert and sociable, while the hyperactive, noisy, assaultive group becomes tranquil. The use of restraints, seclusion, and electro-convulsive therapy has decreased by at least 80% since this study began. Remissions have been produced in 20 patients, and 8 have been discharged. In the near future, leave of absence for all patients in remission is contemplated. As reserpine is used for longer periods of time, we expect remissions to occur in a higher percentage of patients."

Compare Wright(168): "It is seen, therefore, that while 50% of the cases showed a decided improvement, 75% benefited to some extent. . . . Patients showing marked habit deterioration, such as soiling, wetting, and destructiveness, become cleanly, less destructive and better able to care for themselves; and patients given to outbreaks of violence, with a tendency to assault, become much better adjusted to their environment, and their activities are more easily directed into useful channels, following treatment. Agitated, depressed states show much less agitation and take food more willingly; some gain in weight . . . some recover."

given indefinitely. They are not replacement therapies.

Now I hear you say to yourselves, all this is but demolishing straw men. We had long since abandoned such anachronistic empirical organicist practices. The twentieth century has seen the coming of a triumphant new era based solidly on etiology—the era of psychogenesis.<sup>3</sup> I wish that it were so!

Recently I was consulted by the head of the department of internal medicine, who wanted my advice as to what the relationship should be between the psychiatric service and the service of internal medicine. So I rushed to the library and got out a big book(158) on the subject. I did not want to appear to my eminent colleague to be completely ignorant. I began, as is my wont, with the preface. I am not one of those who believe that prefaces are written but not read. Sometimes the preface suffices. It did so in this case because, believe it or not, this is what I read: "No work on psychosomatic medicine could have been attempted without the biologically oriented psychology of Freud." That did it! How could anybody make such a statement who had read Dejerine(37) and Hack Tuke (155), or even Dr. William Sweetser(152) who, in his book on mental hygiene published in New York in 1843, remarked, "Few, we imagine, have formed any adequate estimate of the sum of bodily ills which originate in the mind." Psychosomatic medicine is as old as the hills.

If I had not, like Aurelius Augustine, mis-spent my youth in reading, and had not been cursed with a retentive conscious memory, how much simpler life would be. I might even enter a *gleichgeschaltet* environment with enthusiastic faith and become a successful psychotherapist.<sup>4</sup> The younger generation suffers from no such handicap. In a poll of psychiatric residents, recently conducted in New York, an appalling ignorance of the history of psychiatry was uncovered. It should be pointed out that they cannot depend on histories of psychiatry; they must go to the original texts. As Charles Beard

<sup>3</sup> Whitehorn(161a) "Many now talk about the psychogenic diseases, a trouble-making term whose pernicious implications will plague us for years to come. . . ."

<sup>4</sup> *Difficile est satiram non scribere* (Juvenal).

said, any historian must select his material because the space available is limited and "The moment you select, you begin to interpret." Any history of psychiatry (169) at the present day is apt to be slanted heavily in favor of Freud. One of the brighter of the younger men recently confessed to me that he could read no other language than English. What a frustrating handicap to have to overcome! If he had had a classical education he might have run across the *id* and *superego* in Ovid<sup>1</sup> whose Medea cries (132). If he had had a good Christian upbringing he might have run across the cry of St. Paul: "For the good that I would I do not, but the evil which I would not, that I do (Rom. 7:19). Even in translations he might, as Mullahy (126) says, "have found something of the insights of the Greeks and something of their wisdom, which we are rediscovering at last, twenty-three hundred years later."

Of such a half-educated generation what can be expected? Unless one adopt the attitude of another of the starry-eyed youngsters who, when I asked him whether he had read something of Janet, replied, "Why do I need to read all that stuff? Have I not been analyzed?" He reminded me of the eminent neurosurgeon who remarked to me, "What need have I of neurology? Have I not air and lipiodol?" Such people leave me speechless. Of all the people whom I detest, those whom I detest the most are the ones who believe that they have already entered into Paradise and see all things face to face and not through a darkened glass. But, to return to psychosomatic medicine, the most unkindest cut of all came recently when I read in a small book on psychiatric therapy written in Paris by my friend, Henri Baruk (15): "Freudian psychoanalysis, whose influence has been considerable, has given birth to narcoanalysis and psychosomatic medicine." Is it any wonder that I am bewildered? I remember a remark of that great explorer, Daniel Boone. Toward the close of a long life of wandering around the trackless wastes of the western wilderness someone asked him if he had ever

<sup>1</sup> . . . aliudque *cupido*, *Mens aliud suadet*. *Video melior, proboque; Deteriora sequor* (132).

I hear desire say Yes, and reason No;  
And see with open eyes the better cause,  
And own it better, yet pursue the worse.

been lost. "No," he replied, "but I was a bit bewildered once or twice." I have not been so fortunate. For a long time I have been wandering around the tangled thickets of psychoanalytical literature and I have been often bewildered. I have perhaps only this advantage, that I am no Hanns Sachs (141) to be brought to heel by a frown from my Master.

Although I have read a great deal of psychiatric literature, I never in my wildest dreams imagined that I should one day be invited to deliver this Lecture, so I made no notes. I was given very short notice, therefore I have had to construct it out of the waifs and strays of my recollection, and have spent most of my time trying to identify their parentage. When I had completed it I submitted it to three of my friends, all professors and eminent psychiatrists. One said it was excellent, another said he would enjoy the reactions of the audience, and the third said it would leave me without friends. Do you still wonder that I am bewildered? Well, since psychiatry seems to be largely a matter of faith I can only stand up and bear witness to the faith that is in *me*. I should not have chosen this moment to do so, but perhaps it is for the best. The time of presentation is also short so I shall spend most of it on Sigmund Freud<sup>2</sup> who is most in vogue at present. But you can refer to my recent study of Janet (11).

<sup>2</sup> I do this in the same spirit as Julius Wagner, Ritter von Jauregg. On the occasion of his seventieth birthday Freud wrote to him on behalf of the Vienna Psychoanalytical Society, offering their congratulations. In the course of the letter Freud took occasion to complain of criticism by some of the assistants in the psychiatric department, implying that they had done so at his instigation or, at least, with his connivance. Jauregg replied as follows (see Kolle (107)):

"I seize this opportunity to defend myself against the title of adversary of psychoanalysis. I have for the first and only time, so far as I know, in the year 1930 written something about psychoanalysis and that in a newspaper article in speaking of a work by Alexander and Staub on psychoanalysis in the criminal law.

I believe that whoever may read this essay will not be able to assert that I have acted as an "adversary" of psychoanalytic teaching; I claimed only the right of a critic. For the psychoanalytic teaching created by you is, after all, not a dogma, which depends on faith, but a scientific work which, with all its magnificence, can still be vulnerable in its

Let us begin with Jones' biography (100) which is appearing these days. In its preparation he was obliged to read a great deal. Recently he (99) wrote, "Freud has been regarded as a revolutionary genius who introduced novel and disturbing ideas. The first half of the sentence is undoubtedly true, but the second half needs qualification. As a result of my researches I came to the unexpected conclusion that hardly any of Freud's early ideas were completely new." Then he gives a long list and adds, "This is a formidable list, which I cannot discuss here in detail, and yet it can be shown that there are broad hints<sup>1</sup> of all of them in the writings of previous workers with which Freud was thoroughly familiar." Why was this conclusion unexpected by Jones? Obviously because he had not read of them previously. But there is another aspect of the matter. Why had Freud left a faithful devoted disciple in such ignorance? Freud tells us, in one of his letters to Fliess (63), that he stopped reading because he found his own ideas expressed better than he could. Did he wish to save his disciple Jones from such a deception? Or had he forgotten? If so, what motive had he for forgetting? You will pardon me for asking such a question, but I am told that I must learn to think dynamically—that is, in terms of motives. I leave you to speculate on the answers. I remember only that Freud was a very ambitious man.

In his preface to the first volume of his biography (p. xiii) Jones remarks, "Immeasurably great as was my respect and admiration for both the personality and achievements of Freud, my own hero-worshipping propensities had been worked through before I encountered him." How little even the most eminent psychoanalyst knows of himself! Maybe they had been worked *through* but they had surely not been worked *out*. His biography is one long paean of hero-worship. This was evident even to *Time* magazine. Its very language is evidence enough—"divine

details. For what former students have written against psychoanalysis, I accept no responsibility. I point out only that other gentlemen in my clinic, I name only Poetzl, Schilder and Hartman, have written in the sense of psychoanalytic teaching without my looking askance at them."

<sup>1</sup> See also Brandt (22).

passion, true genius, indomitable courage, imperturbable composure, Herculean labor, extraordinary power, absolute integrity, etc., etc., etcetera" as the King of Siam would have said.<sup>2</sup> Its *Leitmotiv* is clearly apparent on page 53 (vol. I) where he turns Freud's passivity into strength. In this astounding passage one perceives, in the dim dark recesses of Jones' unconscious, the vague outline of a hill in Palestine. Not for nothing does Jones come from a race of preachers. I remember also that it was in the mind of Jones, the only *goy* in Freud's entourage, that there was hatched the idea of organizing a band of faithful disciples.<sup>3</sup> Of course, there were only six instead of twelve, but the idea was the same. Jesus of Nazareth was more fortunate because only one of his disciples betrayed him. Nevertheless, the remaining disciples spread Freud's gospel far and wide.

What was the nature of this gospel? Jones remarks (I, 220) that up to 1893, at the age of 37, "there was little to foretell the existence of a genius." Thus far we can follow him. He further says that "the devising of this method [of 'free association'] was one of the two great deeds of Freud's scientific life, the other being his self-analysis through which he learned to explore the child's early sexual life, including the famous Oedipus complex." He further remarks, "he was on the verge of exploring the whole range of infantile sexuality and of completing his theory of dream psychology—his two mightiest achievements.<sup>4</sup> Eighteen ninety-seven was the acme of Freud's life." We shall remark later on Jones' strange omission of psychoanalytic therapy.

<sup>2</sup> "If one discusses Aristotle, he is the *genius of nature*. If one writes about Plato, he is the *divine Plato*. One does not comment simply upon the works of these men; they are always the works of men veritably divine, men who were the admiration of their century and who received special light from God. It is the same with the subject which one treats: it is always the *most beautiful*, the *most noble*, that which it is the *most necessary to know*." (Malebranche, *De la recherche de la vérité*, II, 6.)

<sup>3</sup> Koestler (106): "Every closed system must of necessity develop an apostolic hierarchy."

<sup>4</sup> Cf. Fromm's (68) list of "the most valuable parts of Freud's teaching: his scientific method, his evolutionary concept, his concept of the unconscious as a truly irrational force rather than as a sum total of erroneous ideas."

Well, I gather from the literature that the theory of infantile sexuality is badly battered. I remember when I first came across the Oedipus complex. Although I thoroughly detested my father, I could not then, or since, find any trace in my unconscious of a desire to murder him in order to replace him in my mother's bed. Others, like William Ellery Leonard(112), in spite of persistent effort, could find no sexual factor in their neuroses. When I lately returned to my interest in matters of pathological psychology, I was much puzzled by this complex. It did not seem to me to fit the Greek myth. Oedipus Rex used to be one of my favorite tragedies. During my student days in Paris, I went often to see it played at the *Comédie française*, just as Freud did. So I got a big book by Mullaly(126) from the library and ploughed laboriously through it.

It was clear as mud but it cover de groun'  
An' de confusion made me brain go roun'.

So, like Belafonte's *Man Piaba*, I sought further and found that not only Horney and Fromm(67) had doubts about the Oedipus complex, but also many others(71). F. A. Weiss(159) lists it as one of the erroneous concepts which must be given up. Even Freud admitted that he had overestimated the primacy of the sexual instinct(66a).

Freud's ideas were often launched with great enthusiasm, like scare headlines in a newspaper, and then quietly dropped without retraction. A good example is the seduction theory of hysteria which is not mentioned in his justification of investigating the sexual life of neurotic patients(Jones, I, 265). Or the retraction may be on the sixteenth page in small print, such as the retraction of the traumatic etiology of neuroses which is buried in the *Civilization and Its Discontents* (p. 96) in a strangely tortured phrase. Speaking of neurosis as the outcome of a struggle between the interests of self-preservation and the claims of libido he says, "Every analyst will admit that none of this even now reads like a statement long since recognized as erroneous."<sup>1</sup>

<sup>1</sup> Whitehorn(160a): "I respect Freud greatly as a scientist, for his hard victory over himself, in proposing to shelve this [libido] theory or at least to postulate something additional in the light of later knowledge and insight." (Cf. also the *Nachträge to Hemmung, Symptom und Angst*.)

It now seems strange to me that I did not realize earlier that many of Freud's psychological writings are not scientific treatises, but rather reveries—a sort of chirographic rumination. From what he told Abraham, it would seem that Freud was the first of the "writing" psychoanalysts who fill the journals with their meditations as a relief from listening to those of their patients(Jones, II, 396). Could much of Freud's writing have been a sort of occupational therapy? He said that he wrote mostly when he had a degree of discomfort of which to rid himself; when he was feeling well he found it difficult to write(Jones, II, 396; I, 305, 346, 356). He wrote, "at high pressure the very few hours he could [Jones, II, 395] spare at the end of a day of toil." There were also months of apathy when he wrote nothing (Jones, I, 306, 344). He could have had little time for revision and correction. If this idea had become evident to me earlier it would have saved me much cudgeling of my brains in an attempt to find in Freudian writings logic and method.<sup>2</sup>

What of the theory of dream psychology? I gather that it, also, is severely battered. Oberndorf(129) writes, "Thirty years later the function and interpretation of the dream remains something of an enigma." Gerard (73), in the last Academic Lecture, expressed some doubt: "Freud's assumption that dreams exist to preserve sleep is not only unnecessary to his important interpretations of latent and manifest dream content and of the nature of dream work; it also seems unsound physiologically." That dreams are wish-fulfillments seems also doubtful to many people.<sup>3</sup> That their symbolism is not

<sup>2</sup> Freud(64): "Since we have good grounds for being distrustful, our attitude towards the results of our own deliberations cannot be other than one of cool benevolence."

<sup>3</sup> Freud(65): "This insight sounds plausible, but it is in the first place too uncertain and lets in the second place more new questions and doubts emerge than we can answer."

Not for nothing was Börne (Jones, I, 246) one of his favorite authors. In his "Art of Becoming an Original Writer in Three Days," Börne concludes: "Here follows the practical prescription I promised. Take a few sheets of paper and for three days in succession write down, without any falsification or hypocrisy, everything that comes into your head." Sounds like James Joyce.

<sup>4</sup> Hoche(84): "From a scientific prosaic view-

established anyone may prove to his own satisfaction by submitting the same dream to several analyzed psychiatrists and comparing the different interpretations.<sup>1</sup> That one's preoccupations appear in dreams in distorted form I can well believe; and that the analyst can make use of this material as a starting point in an analysis just as one can use patients' paintings or creative writing. But this is a far cry from the *Traumdeutung*. I think Jesus ben Sirach(30) was nearer to the truth when he wrote:

He who seeketh vanity findeth delusion  
And dreams elate fools;  
As one watching a shadow and pursuing the wind  
So is he that trusteth in dreams.

This is obviously no place to attempt an appraisal of Freud's psychology, even if I thought myself capable of doing it. Besides, I have no desire either to bury Freud or to praise him, only to protest(79) against his apotheosis. I may ask only why Freud belittled<sup>2</sup> Janet on every possible occasion, though accepting his fundamental hypothesis<sup>3</sup>—a constitutional peculiarity predispos-

point, however, we are dealing with the performance of a play written by no one, which runs off without a stage-manager and without a prompter, and in such a way that some one steps forward from behind the scenes—now from the right and now from the left—and propounds this or that."

<sup>1</sup> Freud(64): "This would seem to be the place, then, at which to admit for the first time, an exception to the proposition that dreams are fulfilments of wishes."

Freud(66): "The old dream-interpreter Artemidoros was certainly right in his contention that the dream changes its sense according to the person of the dreamer."

Freud: "I must, therefore, resort to my own dreams . . . I shall certainly be confronted with doubts as to the trustworthiness of these 'self-analyses' and it will be said that arbitrariness is by no means excluded in such analyses." (Basic Writings, p. 195 Modern Library, 1938.)

<sup>2</sup> Freud(50): "Psychoanalysis soon put itself in sharp antagonism with Janet's opinions because [sic] (a) it refused to trace hysteria directly to a congenital hereditary disposition, (b) it offered instead of a mere description a dynamic explanation by a play of psychic forces, and (c) it referred psychic dissociation [the importance of which had also been recognized by Janet] not to a psychic synthesis arising from a congenital disability but to a special psychic process called "repression" [Verdrängung]."

<sup>3</sup> Freud(55): "As a matter of fact they are serious, constitutionally determined affections. . . ."

ing to neurotic breakdown? Even Jones(98), in his recent Valedictory Address, is forced to admit in neurotic patients what he calls "An innate factor akin to General Intelligence G. It may have to have a physical basis which will bring us back to the often neglected problems of heredity." This comes dangerously near to accepting the ideas of a powerful rival of his Master. Also Freud(57), in his *Analysis Terminable and Interminable* talks pure Janet without, of course, giving credit. The analysts begin only now to do grudging justice to Janet. S. Nacht(127) of the Psychoanalytical Institute in Paris says, "Psychic causality is only a causality of second degree; the psychic conflict does not acquire a pathogenic value except on certain terrains." P. Male(117) also places emphasis on the neurological terrain.<sup>4</sup>

That many of Freud's theories were ill-founded<sup>5</sup> and have been abandoned, partly by the founder himself and more by his disciples, is now well known. F. A. Weiss(159) lists, as ideas which must be given up, the following: The priority of sexual events in childhood, the castration complex, penis envy, the Oedipus complex and cure by *Abreaction* of old trauma. How could it be otherwise when we remember how Freud built up his theories? (Jones, I, 45). Just before his seventieth birthday he remarked that he had spent his life guessing how the mental apparatus is constructed. He sat in his study, pondered on these matters and scribbled his ruminations. Unfortunately that is all he did. Thrice in his early life he essayed the experimental method and thrice he failed (Jones, I,

<sup>4</sup> Jung(101): "Disturbances in the sphere of the unconscious drives are not primary, but secondary disturbances."

"A neurosis is a dissociation of the personality."

<sup>5</sup> Freud(66): "The deficiencies of this small, more preparatory than finished, essay are perhaps in small part excusable if I give them out as unavoidable. In a few sentences about psychic consequences of the adaptation to the reality principle, I must announce meanings that I should rather have retained and whose establishment certainly will cost much effort."

Freud(66): "But I am myself ready to admit willingly that such wide-ranging conclusions as the above should be built on a wider basis."

Freud(66a): "It is almost shameful that, after so much labor, we should still find difficulty in conceiving of the most fundamental matters. . . ."

54). These are the failures which Jones says laid the foundation of his genius.

I know that there are attempts to prove that psychoanalysis is a science.<sup>1</sup> They do not convince me and have convinced very few objective observers (146). Even Freud (56) admitted that it is only a sort of postdictive science, lacking in power of synthesis and prediction. Science cannot be built on the insights of visionaries or on the mutual titillation of interdisciplinary minds at Palo Alto, or elsewhere. Science can be built only by the cautious, laborious verification, step by step, of one's hypotheses, establishing each one solidly before passing on to the next and, as Jones says (1, 34, 40), Freud had no patience with such a method. It is not a glamorous process. I remember one day sitting in the front row at a meeting of the American Neurological Association when Smith Ely Jelliffe came in, listened for a few moments, then turned to me and remarked, "Oh, Bailey, I'm sick of this deadhouse stuff. I wish a pretty woman would come in and tell a dream."

In his *Totem and Taboo* Freud (58) says: "Though my arguments have led to a high degree of convergence upon a single comprehensive nexus of ideas, this fact cannot blind us to the uncertainties of my premises or the difficulties involved in my conclusions . . . . It must be admitted that these are grave difficulties, and any explanation that could avoid presumptions of such a kind would seem to be preferable." We can agree with that one too. In another place (54) he said, "One might ask me whether and how far I am convinced of the correctness of the assumptions here developed." My answer would read that I am neither myself convinced nor do I ask

<sup>1</sup> Zilboorg (171): "Psychoanalysis rests on individualism, empiricism, and the inductive method. Much in the libido theory will be revised, but its fundamental postulates will remain."

Dixon (40): "In an authoritative scientific work of the date 1905 I read not long since the sentence, 'Ether is the fundamental postulate of physics'. In an equally authoritative work upon modern physics, dated 1934, I could not find the word 'ether' at all; the 'fundamental postulate' did not even occur in the index."

The postulates of any science are as subject to scrutiny as the conclusions derived from them. In the present case the result may not be psychoanalysis but it might be nearer the truth.

that others shall believe them; or, better stated, I don't know how far I believe them."

It has become a habit, in beginning a psychiatric lecture, to pay tribute to Freud's genius. Thus Montagu (125), in his dinner address to this Association, remarked: "This general [pessimistic] viewpoint has received what is perhaps its most striking reinforcement from a source which undoubtedly represents the most insightful contribution to our understanding of human nature in the history of humanity. I refer to the psychoanalytic theories of Sigmund Freud." He then proceeds to demolish the viewpoint. This is an old procedure; Freud (166) complained bitterly of it. Still, his teachings continue to arise like a phoenix from its ashes. Lately a psychologist was overheard at the Chicago State Hospital expounding for the delectation of the affiliate nursing students, on whom we spend thousands of dollars every year to bring them to our hospitals, the discredited theory of infantile parental incest in all its pristine naïvety. When one remembers how the teaching of Franz Josef Gall still colors all our neurological thinking one wonders how long the hoary errors of Freud will continue to plague psychiatry.<sup>2</sup>

In my youth I read a great deal in the writings of Thomas Aquinas. I was greatly intrigued by the apparent geometrical solidity of his argumentation. In those days I could read it in the original Latin. It was beautifully constructed and seemed to me logically conclusive until one day I read how Siger of Brabant (118) pointed out to him that no amount of logical argumentation could reach any conclusions which were not embedded in the premises. Poor Thomas bellowed like a great Sicilian ox at the thrust and never recovered. I stopped reading the *Summa Theologiae*. Why then should I occupy my time in reading the writings of a man who doubts his own premises and chooses<sup>3</sup> from a mass

<sup>2</sup> Whitehorn (160a): "I sometimes wish that some of his admirers who have a talent for popular exposition would not have perpetuated so interminably and so widely some of his wrong guesses."

<sup>3</sup> Sasz (143): "Thus, for the sake of making his point about psychoanalysis, Freud chose to stress the similarities between it and certain purposes of the law."

Freud (58): "Even the account I have just given, derived from the book published by Frazer in 1887,

of data of observation those items which support his thesis? This is the procedure of a lawyer or propagandist, not of a scientist. St. Thomas never doubted his premises (4). He regretted only the years he had spent trying to prove what was a matter for faith not logic. On his deathbed he asked (31) to have read to him the Song of Songs Which is Solomon's, much to the scandal of his faithful friend, Reginald.

Psychoanalysis is called a science. So have I read books on the science of metaphysics. There is also a Christian Science (156). This comes of confusing incompatible things, just as St. Thomas confused faith and logic. If psychoanalysis were a science, the analysts would, ere this, have merged into the academic community where there are numerous intelligent men as tolerant and sincere as themselves. But if, as I fear, they have only a faith to defend they will make every effort to maintain their separate institutes in which they can mutually reinforce their conviction of unicity. Of course, in this withdrawal they are only following the example of their Master. Could Freud have feared that he might encounter a Siger of Brabant? Or was Krafft-Ebing his Siger? (Jones, 1, 263).

Even the term psychoanalysis cannot be defined in such a way as to please a majority (33). Oberndorf (129) says: "The difficulty of exact definition must be bewildering to the general public." I reckon I belong to the general public. Yet those who call themselves psychoanalysts have moved into the seats of the mighty.<sup>1</sup> Gone are the days when

is open to the criticism that it expresses the present writer's arbitrary preferences; and indeed it would be contested to-day by Frazer himself, who has repeatedly changed his opinions on the subject."

Freud (59): "It was my good right to select from ethnological data what would serve me for my analytic work."

<sup>1</sup> Lawrence (113): "Psychoanalysts know what the end will be. They have crept in amongst us as healers and physicians; growing bolder they have asserted their authority as scientists; two more minutes and they will appear as apostles."

Jones (98): "How many years will pass before no Foreign Secretary can be appointed without first presenting a psychoanalytical report on his mental stability and freedom from complexes?"

Laforge (110): "I need this freedom not to protest or to express but to support those young psychoanalysts who wish to remove themselves from

analysts were segregated. The disdained have become more intolerant than their predecessors. They say they agree that a man can be a good psychiatrist without having been analyzed. But they do not act as though they believed it. They have made the younger generation believe that, if you have not been analyzed, you belong to a lesser breed. The youngsters believe that, if they are not able to talk the special language of the system, they are apt to have trouble with some of the associate examiners of the Board of Psychiatry. It is not enough to speak dynamically. I know at least two other dynamic systems (44, 93) besides the Freudian, and many heretical variants of the Freudian system, yet a candidate has a good chance to draw an examiner to whom dynamic psychiatry and psychoanalysis are synonymous. "Verily, the stone which the builders refused has become the headstone of the corner" (Psalms 118: 22).

Yet there is no proof that the system is true. It is an intellectually closed world, but the argument that it is internally coherent is no proof, as we have just noted, and is, moreover, demonstrably false. Another proof sometimes adduced is its triumph over opposition. This is the proof theologians sometimes use to prove the truth of Christianity which the analysts disdain. It can be used with equal cogency to prove the truth of Mohammedanism or Buddhism. Oberndorf (129) remarks on "The profuse number of new books, based upon psychoanalytic thinking, which appear weekly." Of the making of books there is no end; Swedenborgian and Rosicrucian books are still written. John

the power politics which goes on under cover of orthodox psychoanalysis."

Zilboorg (170): "Power is always a rather unsettling component of human functioning. As every psychoanalyst knows, power is a corrupting component in human behavior . . . Psychoanalysts must learn to give up functioning with power."

Menninger, K.: "It must be recalled that Freud had always serious fears about the excessive popularity of psychoanalysis in America. He was afraid to see his essential goal diluted and compromised. Many of us who practice psychiatry and psychoanalytic teaching have partaken of this fear in recent years although for different reasons." (Psychiatrie et psychanalyse. Pp. 82-4 in *La Psychiatrie dans le monde. Encyclopédie médico-chirurgicale*. Paris, 1955.)

Morley(36) noted that "The wholesome gospel [of phrenology] was circulated by scores of thousands of copies." Another proof of which one hears is its therapeutic efficacy. This is the weakest proof of all. Freud(166) remarked toward the end of his life that psychoanalysis would be remembered as a psychology of the unconscious and not as a method of treatment. Brill(21) said that psychoanalysis has a very limited therapeutic applicability. Horney(85) wrote, "My desire to make a critical revision of psychoanalytic theories had its origin in a dissatisfaction with its therapeutic results."<sup>1</sup>

Horney perhaps did not realize that the effects of psychotherapy seem to be entirely independent of the ideational system on which it is based.<sup>2</sup> In the GAP report No. 31 (March 1955) it is stated: "The subtle interpersonal experiences which make up the

<sup>1</sup> Freud(66): "Psychoanalysis gives nothing therapeutic in the severe forms of real mental disturbances."

Freud(51): "You know that the theory of psychiatry has hitherto not been able to influence obsessions. Can psychoanalysis perhaps do so, thanks to its insight into the mechanism of these symptoms? No . . . it cannot; for the present, at least, it is just as powerless in the face of these maladies as every other therapy."

Gitelson(74): "Its range of strict application as regards the gamut of mental illnesses is limited."

Jones(98): "To achieve anything like complete freedom and inner harmony by means of psychoanalysis is even harder than we sometimes like to think."

<sup>2</sup> Adolf Meyer(32) said: "When the patient and the doctor agree as to the cause of the illness, the patient is cured."

Freud(120): "Psychoanalysis was then first and foremost an art of interpreting. Since this did not solve the therapeutic problem, a further aim quickly came in view: to oblige the patient to confirm the analyst's construction from his own memory."

McCulloch(120): "Where written words remain to check our recollections they often prove us wrong. We rewrite history, inventing the past so it conforms to present needs. We forget, as our machines forget, because entropic processes incessantly corrupt retention and transmission of all records and all signals. Partly because all men, when pushed, fill in the gaps of memory . . . findings of repressed unconscious stuff rest on confabulation . . ."

Jones(100): "Freud's memory, like everyone else's, could be treacherous at times, and the contemporary data enable one to render more precise, or even to rectify, the accounts of events he described many decades later."

psychotherapeutic processes are difficult to concretize and verbalize. Ordinary scientific scrutiny and assessment are not readily applicable to the effects of psychotherapy. The many schools and methods of psychotherapy seem to achieve roughly comparable results." In summarizing the great meeting on Psychiatric Treatment in 1953, S. Wortis(167) wrote: "It would also appear, as one looks over the data of the cause of personality illness and the effectiveness of various therapeutic procedures, that approximately one-third get well with treatment and one-third may be influenced to a greater or lesser degree by treatment. We have only meagre information concerning what happens to patients who are not treated." This checks with Oberndorf's(130) statement that 30% of his patients have previously been analyzed without success. And Dr. Denker(38) reported that 72% of psychoneurotics recover within two years with no other assistance than that of a general practitioner of medicine. Esquirol reported identical results 120 years ago. There seems little doubt that the results of psychotherapy are independent of ideology. The most successful psychotherapist(87) whom I know relaxes his tense patients on the basis of a purely physiological theory.

In the specific instance of psychoanalysis there has been a great reluctance, says Oberndorf(129), on the part of the analysts to submit data. This has been confirmed by the Psychoanalytical Association(33). D. Levy(114) in his Academic Lecture tells of a social worker who was totally uninterested in the results of her therapy. This one may understand of a social worker, but why should physicians react in that way? Since I am now trying to think in terms of motivation, I may change the question, "Why is the psychoanalyst reluctant to check the results of his therapy?" Could it be that he fears definite confrontation with failures would shatter his faith in his method and thus impair his efficiency? William Brown(25) wrote that he got good results with several methods so long as he had faith in them. Will any faith do, even faith in psychoanalysis? Freud(55) implied as much when he said, "As to that I may say that I do not think our successes can compete with those of Lourdes. There

are many more people who believe in the miracles of the Blessed Virgin than in the existence of the unconscious." That there is some lack of faith may be indicated by a slip of the tongue on the part of an eminent psychiatrist at the aforementioned meeting. He kept talking of "unrecovering" techniques. Freud has taught us that such slips have meaning. Recently Berman (18) has written that it is "necessary to understand the therapeutic process more fully before very meaningful studies of the results of [psychoanalytic] therapy can be expected.

All this makes me wonder concerning the value of psychotherapy in general. Whenever a true experimental test is set up, as in the Cambridge experiment, the results have been disconcerting. Teuber (154) thinks that "the data yield one definite conclusion: That the burden of proof is on anyone who claims specific results for a given form of therapy." Objective methods of evaluation are being sought (83) but are not yet available. Meanwhile I do not wish to imply that psychotherapy is of no value. An old medical adage says: It is a physician's privilege to cure seldom, to relieve often, to comfort always.

This is a matter of practical importance to me at present. Most of the psychotherapy in the state hospitals is done by psychologists. There is a great pressure to appoint more of them. Some psychiatrists disapprove of their doing psychotherapy; the psychoanalytic association formally condemns the practice. Nevertheless, the present conflict over the right to do psychotherapy can be laid directly at the door of the psychoanalysts.<sup>1</sup> By their neglect of, and disdain for, the biological and medical aspects of mental disease, they have encouraged nonmedical people to enter the field who can call Freud to their support. He (52) wrote: "I strongly emphasize my demand that nobody should be permitted to practice psychoanalysis unless he has ob-

<sup>1</sup> "Unfortunately, neither [Freud nor Meyer] has fulfilled the original hopes. Further, in that psychoanalysis came to ignore the physical factors in mental illness, and both ignored, or almost ignored, the hereditary and constitutional factors, they succeeded in diverting attention from some very important aspects. By this shift of the emphasis some erroneous conceptions have been fostered with the development of some popular fallacies." (Curran and Partidge (32)).

tained this privilege on the basis of thorough training. Whether such a person is a qualified physician or not does not seem important to me."

The psychologists say that, if they are forbidden, no psychotherapy will be done in the state hospitals. Would this be a calamity? How much does psychotherapy accomplish with psychotic patients?<sup>2</sup> The great problem of the Mental Health Service, says Johnson (97), is schizophrenia. What have the psychologists contributed to the solution of this problem? The best that Whitehorn (160) could say for it was "The case is not wholly for or against psychodynamic principles." I know of no direct data bearing on the question but perhaps we can get at it in an indirect way. If the results obtained by psychotherapy were spectacularly successful the superintendents of overcrowded asylums would make the rafters ring with their clamor for more psychotherapists. The conspicuous absence of such clamor indicates to me that psychotherapy is not apt to relieve our overcrowding significantly. There is a great need for more psychologists in our hospitals to do psychological work, but as therapists, I doubt that an increase of the number of psychologists would accomplish any more than an equal number of occupational and recreational therapists. This doubt is supported by the study of the State Charities Aid Association of New York City. How could it be otherwise? The incidence of schizophrenia remains constant regardless of race, climate, creed, or mental hygiene. Whitehorn (161) says: "Much of the material disclosed is not inherently peculiar to schizophrenia, and it has only an indirect bearing on the schizophrenic illness." Henri Ey (45) has remarked to what banalities the psychoanalysts are reduced in their attempts to account for it by psychogenesis.

<sup>2</sup> Oberndorf (129) has summed up our progress to date very succinctly as follows: "One change is noticeable. Then, among the notes of the chronic patient's condition, one would be apt to find at not too frequent intervals: 'The patient is disinterested, apathetic, sits alone, will not work and is deteriorated.' In contrast to this, in 1950, the notes read: 'The patient is disinterested, apathetic, sits alone, will not work and is regressed.'"

*Cf. also Alexander, L. Treatment of Mental Disorder, Chap. 12. Philadelphia: Saunders, 1953.*

The shortcomings of psychiatric therapy have been shrewdly analyzed by Alan Gregg (76). Freud never believed that psychotherapy could treat schizophrenia (55) successfully but analysts use Freud's writings as theologians use the Bible; they take from them only what suits their purposes. In this again they are only following their Master's example. This squabble over psychotherapy arouses another echo from the past. There was published in 1845 by Baron Ernst von Feuchtersleben (46), Dean of the Medical Faculty of Vienna and Secretary of the Imperial and Royal Society of Physicians, a discussion which sounds as if it might have been written yesterday.<sup>1</sup>

If the results of psychotherapy are independent of the underlying ideology of the

<sup>1</sup> Feuchtersleben (46): "There is something in the ever-recurring squabbles between the best writers on our subject which excites a compassionate smile in those who are in the habit of examining questions calmly and impartially. Who denies, who can deny that often, and independently of bodily causes, erroneous notions, unbridled passion, overpowering feelings, or a want of development may change the regular course of psychical operations in such a manner, that it may justly be said that such a mind is diseased? Who can deny that such a disease is not to be removed by cold showerbaths, tartar emetic, etc., but wholly and solely by an influence on the mind? But, if we impartially weigh the subject and extent of medical art and science, do we not immediately perceive that every psychological physician, who treats the abovementioned condition in the abovementioned manner, is called a physician only in a metaphorical sense? The question in dispute is, properly speaking, not whether the mind can become diseased, but whether the task of treating independent states of the mind by education, instruction, etc., is to be considered as belonging to the province of the physician or not? The present state of the world seems to reply in the negative, because, since these moral influences are confided to parents, teachers, the clergy, etc., and by diseases, in a non-figurative sense, only the somatic are understood, the physician has to do with them alone. But here, as in human knowledge in general, it happens occasionally that these moral and logical conditions, which, as well etiologically as therapeutically, are wholly independent, trench on the somatic, and enter into the domain of the physician, whose rule, therefore extends over them. The confines, as in all human knowledge, touch without effacing each other, and this is the main substance and object of all medical psychology, so that the physician, who is wholly unacquainted with the relations of intellectual to physical life will not be able to comprehend and treat the latter in all its various bearing."

therapist, what is the secret of its success? There seems to be a considerable body of convergent opinion on that question. Ferenczi said that the sympathy of the physician cures the patient (110), and over this matter diverged from his Master and followed Dostoevski who wrote "Only active love can secure faith for us." Ross (139) said, "When belief is present in abundance then the theory works." Charcot (29) said, "It is faith that heals." Dejerine (37) said, "From my point of view, psychotherapy depends wholly and exclusively upon the beneficial influence of one person on another. . . . There is something analogous to faith in this. . . ." <sup>2</sup>

<sup>2</sup> Wm. Brown (24): "Turning now from these wide generalizations to the details of mental analysis, what one finds in analyzing patients is that practically all cases need a philosophy of existence. Whilst they need help in regard to the individual problems of their lives, they need also to be shown that life is worthwhile and shown what its worth is or may be. Sooner or later in the course of the analysis the patient brings up the question of religion, and my impression from the psychotherapeutic point of view is that religion is deep-seated in every mind."

"We are here dealing," as Masserman (119) puts it, "with the deepest of dynamic insights about man's own behavior. Modern psychiatry is, indeed, just beginning to accord proper recognition to the significance of these facts—for who can label as illusions concepts as nearly fundamental and universal as those we have discussed? Perhaps only now can we really appreciate the unconscious depth and tragic, infinite yearning expressed in the Dostoevskian maxim: 'Man must believe in his gods, in his fellow man and in himself,' for without these deepest of all Ur-defenses man, in intolerable anxiety, would indeed perish."

Freud wrote to the pastor Pfister "From a therapeutic point of view I can only envy you the possibility of sublimation that religion affords" (Jones, II, 199).

Goethe: "The real theme, the sole and fundamental theme of the history of the world and of man, the theme to which all others are subordinate, remains the conflict between belief and disbelief" (Quoted by Paulus Lenz-Médoc in *Satan*, Sheed and Ward, N.Y., 1952, pp. 495-96).

Jung (101): "How can he [the physician] help the sufferer to attain the liberating experience which will bestow upon him the four great gifts of grace [faith, hope, love, and insight] and heal his sickness?" The greatest of these is not insight. [Now abideth faith, hope, love, these three, and the greatest of these is love (I Cor., 13, 13)].

"[Freudian psychology] points no way that leads beyond the inexorable cycle of biological events. This hopelessness would drive one to exclaim with

If this be the case it is difficult for me to understand how a psychoanalyst can inspire faith. Ostow (131) asks: "Granting that psychoanalysis can rid us of unconscious guilt and so usurp the old office of religion, what is there in psychoanalysis to inspire optimism and morality?" Yet Jones (98) says he sought something which was "more likely than any other to yield the secrets that were perplexing me, about the nature of the soul, the purpose of life and the means of controlling our animal nature." He seems to have found in psychoanalysis, if not answers, at least opiates, and Hanns Sachs (141) tells how "When I finished the Book [*Traumdeutung*] I had found the one thing worth while for me to live for." It is amazing what men find to live by!<sup>1</sup>

Jung (101) says that "Among all my patients in the second half of life . . . there has not been one whose problem in the last resort was not that of finding a religious outlook on life." This was one of the reasons for his separation from Freud. What sort of help could such a patient expect from a psychoanalyst whose Master taught that religion is an illusion? Freud had no help to give such patients. Jones said that "He grew up devoid of any belief in a God or Immortality, and does not appear ever to have felt the need of it." This is rank nonsense. In one of his letters Freud writes that these enigmas "suddenly assail one in the morning and rob one of one's composure and one's spirit." (Jones,

Paul, 'Wretched man that I am, who will deliver me from the body of this death?'

<sup>1</sup> What Hanns Sachs (141) was looking for is betrayed by his statement "All I want is not to pose as the disciple who leaned on the Lord's breast," a reference to the Apostle John.

This reminds me of one young psychiatrist, to whom I had given Jones' account of the fifteen theses of Freud which were not original to him, who said defiantly, "There is one thing you cannot take away from him—the discovery of the phenomenon of transference." I smiled and said nothing but I reflected internally that the whole Christian religion rests solidly on the phenomenon of transference. "Verily I say unto you, Whosoever shall not receive the Kingdom of God as a little Child, he shall not enter therein." (Mark 10:15). "Like as a father pitith his children, so the Lord pitith them that fear him." (Psalms 103:13). "Come unto me all ye that labor and are heavy laden, and I will give you rest." (Matthew 11:28). What is this but an invitation to transference?

I, 175). In his *Moses and Monotheism* he (59) remarked that he envied those who have a religious faith. Puner (137) says, "His concern and his lifelong preoccupation was as much with the mystical domain of the soul as any theologian's or witch doctor's.

How little Sachs had to offer a troubled analysand is evident from his remark to Boring (20) at the end of the analysis when Boring expressed some apprehension about his ability to go on from there. "Where there's a will, there's a way." The futility of such a statement would have been immediately apparent to Janet who had long ago shown that one of the characteristics of neurotic patients is their inability to synthesize. It is not that they *do not* will, but that they *can not* will. This is why many keep coming back and the analysis ends only in mutual exhaustion, as Sullivan (151) says, of the two parties. Freud (141) was so annoyed by this that, in a moment of exasperation, he called them fools. In his *New Introductory Lectures* he (55) wrote "As a matter of fact they [the neuroses] are serious, constitutionally determined affections, which are seldom restricted to a few outbreaks but make themselves felt as a rule over long periods of life, or even throughout its entire extent." I wonder how many analysands go away from their séances with just such evasive advice, while their analysts, over their cocktails, argue about metapsychology.<sup>2</sup>

Of course, Freud (55) at first believed that

<sup>2</sup> Here comes another echo from the past. St. Augustine (5) tells, in his treatise on the Magnitude of the Soul, how he answered one of his pupils, who had asked him an embarrassing question, by telling the boy that he would understand better when he had studied further. "But, what I said to the boys when they looked at me, eager for an explanation, I am afraid to say to you now, for we have already gone so far that, unless I give you a different answer to support my case, our attention, after weathering the barrage of so many words, may appear to have succumbed to the bite of a single worm. I advised the boys to continue their studies, as they had begun, and thus they would come at the right time to search out and learn the answer to these problems, if they warranted an answer. But what I said to Alypius, as the boys went away, and both of us, each in his own way, fell to sifting and spinning out hypotheses in our search for an answer, if I wished to explain all this, it would call for more words than we have used in this dialogue from its start, with all its meanderings and digressions."

the patient, once he understood his difficulties, would be able to overcome them himself and many analysts still seem to believe that, if only they can probe deeply enough, all will be well. They talk hopefully about depth therapy. Yet experience has proven (80) that deep psychotherapy is as dangerous as deep surgery. The technic of deep analysis seems to be to lead the patient along the very brink of the abyss, hoping that he will not fall in; something like Dulles' diplomacy.

In psychoanalysis I can find no vision<sup>1</sup> without which the people perish. *Frustra de profundis ad psychoanalystes clamavimus*. Then why does it flourish? It is all very bewildering. It has been banned in the Soviet Union because it treats society not as something which creates new forms of psychic life, but as a negative force which suppresses man's basic needs (17). It has not yet been denounced by the Pope but the thunder begins to rumble along the Tiber (136). Its Master neglected and disdained women—half of the human race—and admitted that he never understood them (39). He never appreciated the usefulness of music yet, as one analyst (119) puts it, "What better wings on which to soar from the pedestrian round of earthly living?" He completely ignored all spiritual values (150). He neglected the social nature of mankind (69). He developed no system of values (170). He neglected the fundamental instinct of curiosity, so characteristic of all primates (27), as well as many others (157), more powerful in human motivation than the sexual instinct, which have lifted us above the Bandar-log. He over-emphasized the unconscious as compared with what G. H. Mead (121) called "that type of intelligent conduct which is peculiarly characteristic of the higher forms of life, and especially of human beings." He taught (53) that civilization decreases happiness by increasing the feeling of guilt; he had never lived in a primitive society. As Masserman puts it "He let his own formulations influence his therapy in the direction of pessimism, conservatism and, occasionally, even covert nihilism."

<sup>1</sup> Alexander (1): "If we believe in this cultural function of our discipline and if we believe that our aim can be reached, this will serve as a vision which is the moving power behind every productivity."

When charged with some of the deficiencies<sup>2</sup> in his work Freud (166) replied, "That's the sort of criticism I often hear from the Bolsheviks. I can't discuss everything." True. We should remember the lopsided<sup>3</sup> incompleteness of his work and not conclude, as many of the young psychiatrists seem to do (70) that all else is unimportant. Freud (56a) summed up his work very well when he wrote, "I can say that I have made many beginnings and thrown out many suggestions. Something will come of them in the future. But I cannot tell myself whether it will be much or little." Nor can we. At the end of his life, as the shades of eternal night gathered about him he made a pathetic attempt to sum up his work, playing the record over in the old well-worn grooves—the structure of the mind (which he spent his life guessing at), the meaning of dreams

<sup>2</sup> Fromm (69): "Freud observed three facts, and each of these observations was valid. We now propose to show that the unified theoretical interpretation which he gave to his three observations was fallacious and that the progress of psychological theory lies in the direction of seeing the observed phenomena afresh and of interpreting them differently."

Deutsch (39): "Women like her are a living refutation of Freud's assertion that a feminine woman does not love but lets herself be loved."

Piaget (134): "The problem of intelligence is, in fact, absent from Freudism and it is too bad, because meditation on becoming conscious in the act of comprehension, as well as on relationships between the unconscious intellectual schemes and conscious reflection, would certainly have simplified the theory of the unconscious affect."

Bauer (17), quoting Zalkind: "The Freudian can tell why a person is the way he is, but can give you little help in making him what he should be. Furthermore, Freudianism puts too much emphasis on internal, unconscious processes, and too little on conscious processes and man's relationship to society."

Stern (150): "It is the tragedy of psychoanalysis that it was evolved by a nineteenth-century scientist who was very careful to remain what one used to call 'scientific.'"

Zilboorg (170): "It [psychoanalysis] has precious little to do with regard to the essential fundamental issues of values, temporary and eternal, social and individual, public and personal."

Jung (102): "Freud's teaching is definitely one-sided in that it generalizes from facts that are relevant only to neurotic states of mind; its validity is really confined to those states."

What Freud has to say . . . can be taken as the truest expression of his own psychic make-up."

and the Oedipus complex (which Jones says are his most important contributions) and one brief section on technic, giving thus eloquent expression of his belief that psychoanalysis would be remembered as a psychology of the unconscious and not as a method of treatment (166).

In spite of all this, I sometimes am surprised to find in my bosom a sort of grudging admiration for Sigmund Freud. Dr. Beep, professor of theology at the Catholic University of Freiburg, stated: "Freud is a fanatical searcher for the truth and I believe he would not hesitate to unveil it though it should cost him his life."<sup>1</sup> It takes courage of no mean order to continue to peer defiantly into the mysteries of life and death. Nothing is more difficult for a human being than to maintain an attitude of suspended judgment when the chips are down and eternity is at stake. Yet how many neurotic patients are capable of such a stoic attitude? Freud himself suffered from "a very considerable psychoneurosis" (Jones, I, 304) and was often on the verge of despair. Much as one may admire his courage, one need not follow his example. We are inclined now to disdain the Russian psychiatrists who idolize Pavlov (163). Must we bow down before *dem goldenen Sigi* (Jones, I, 3)?

The great revolution in psychiatry has solved few problems (41). Revolutions do not necessarily bring progress. There has been a great revolution in Russia; are the people better off? We are not convinced. Are the mental patients better off as a result of the great psychiatric revolution? I wonder.

Myself when young did eagerly frequent

Doctor and Saint, and heard great argument  
About it and about: but evermore

Came out by the same door as in I went.

I understand now why, in the last years, I have a recurrent dream that I am wandering through a dense pathless forest but never arrive anywhere before I finally awake. Lately I have another recurrent dream: A vine, seeking in vain a support on which to climb to higher things, twines around its own base.

<sup>1</sup> These are very like the words of a letter which Schopenhauer wrote to Goethe (see Guthrie (81)). "The philosopher must be like Oedipus—pursue his indefatigable inquiry regardless of the horror which the answer holds."

Something further may follow of this soliloquy but it is not probable. Futile inactive contemplation of the mysteries of life is not part of the Western *Weltanschauung*, which was very succinctly epitomized for me by my master—Harvey Cushing. Shortly before his death I visited him. He was very busy with his bibliography of Vesalius. He looked very wan and frail and I remarked to him that, when the bibliography was finished, he could relax and rest. "No, Bailey," was his prompt reply, "the only way to endure life is always to have a task to complete." The task for the psychiatrists,<sup>2</sup> it seems to me, is to get back into the asylums and laboratories (119a, 116) which they are so proud to have left behind them, and prove, by established criteria, that their concepts have scientific validity (138).

When this Lecture began to take shape in my mind I feared that the spectacle of my bewilderment might have a tendency to cause dismay among the young psychiatrists and impair their effectiveness but I soon realized that this was a bit of unwarranted egotism on my part, for am I not a Child of the Darkness (42) whereas they are all enlightened? Yet out of the darkness of my bewilderment has come a faith that the problem of schizophrenia will be solved by the biochemist. I cannot work on this problem since, alas, I am only a surgeon. I shall, therefore, continue to spend my remaining years and strength at what Freud (166) told J. Wortis was an important problem for the future—the relation of the physical to mental states,<sup>3</sup>

<sup>2</sup> Appel (3): "I believe this can best be accomplished by a closer rapprochement between analysts and psychiatrists, as full-time men on medical school faculties. . . . The isolation of analysis from medical clinics, hospital, and faculties hinders its wholesome and effective development."

Jones (98): "The idea of an independent profession which some analysts cherished a quarter of a century ago is one that belongs to the past."

Whitehorn (Am. J. Psychiat., Aug. 1952, pp. 81-88): "In the main, however, the psychoanalytic institutes operate separately from the universities. I am frank in stating my opinion that this arrangement appears to me anomalous and unsound."

<sup>3</sup> McCulloch (120): "But Breuer, who at first agreed with Freud, would not be persuaded of Freud's elaborations. Freud felt rejected and again, as with Bruecke, Meynert and Charcot, the father-surrogate was finally rejected, despised, repudiated."

It is interesting to find Freud returning to the

the problem on which his teacher, Meynert, spent his life (123), and I shall give all the support I can muster to the biochemists and biophysicists.<sup>1</sup> In this determination I am supported by two of the most gifted probbers into the unconscious. Pierre Janet (90) said that he expected the problem of schizophrenia to be solved by the chemist. When asked why he did not work on this problem he replied that, alas, he was only a psychologist. That this is an insufficient excuse is proven by Heinrich Klüver (104), a distinguished psychologist, who has made significant contributions to the chemistry of the brain. You will remember also that Sigmund Freud was a failure as a chemist but, at the end of his life, he (61) said: "The future may teach us how to exercise a direct influence, by means of particular chemical substances, upon the amounts of energy and their distribution in the apparatus of the mind. It may be that there are other undreamed of possibilities of therapy. But for the moment we have nothing better at our disposal than the technique of psychoanalysis and for that reason, in spite of its limitations, it is not to be despised."<sup>2</sup>

problem on which Meynert expended all his abundant energy.

Meynert (123): "The main function of the central organ is to transmit the fact of existence to an ego gradually shaping itself in the stream of the brain . . . If we look upon the cortex as an organ functioning as a whole then the information that it subserves the processes of the mind is all that can be said . . . To think further about the cortex is impossible and unnecessary . . . But our hope to understand eventually the functions of the hemispheres is raised again by the opposite assumption which leads us straight to an organology of the central surface . . . Between these two theoretical possibilities the facts have to decide."

One hears again the voice of Franz Josef Gall.

<sup>1</sup> I speak now from the standpoint of the physician. The demonstration of a chemical factor in the causation of schizophrenia would not help us to understand the contents of schizophrenic delusions, just as the demonstration of the spirochaete in general paralysis did not help us to understand the contents of the megalomanic delusions of the paretic; it would merely make them superfluous.

<sup>2</sup> Freud (64): "They [the biological answers] may be of a kind which will blow away the whole of our artificial structure of hypotheses."

Freud told Schilder, when he was starting to America, that he should hurry to study the psychology of the schizophrenic before someone found an injection to give them and they become as rare

as the American Indian. (Quoted to Heinrich Klüver). According to Jones, he made a similar remark to Marie Bonaparte. Cf. also Ophuijsen J. Clin. Exp. Psychopathol., 12: 1, 1951.

McCulloch (120): "The remedy lies not in government, already overgrown, but in giving doctors tools with which to help their patients. Only slow science can do that."

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## THERAPEUTIC FORCES IN EARLY AMERICAN HOSPITALS<sup>1</sup>

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A look at the past seems useless unless it has bearing on the present and future. It is precisely now, in 1956, that we need the revival of principles and practices, which were known long ago, to serve as a solid foundation for the employment of promising new therapies. Together old and new would justify a controlled but well-founded optimism.

I confine my report to hospitals run by the 13 (really 10 active) Founders of this Association in the years 1844 to 1856: this means, of course, that I report on hospitalized psychoses only.

A first therapeutic force was space; that is, "no overcrowding which becomes an apology for what otherwise would not be tolerated." Space which allowed each individual to have some privacy and to benefit from individual attention from doctors and attendants. All the century-old hospitals enjoyed space for a while; then came a tendency to crowd in patients without expanding quarters or the number of doctors and attendants, until at present there is the cry—"not enough of anything except patients!" A century ago there was usually 1 attendant to 4 or 8 patients.

By space the Founders meant a proper proportion between hospital space, measured in cubic feet, attendants, employees, and physicians—a powerful therapeutic force in itself.

A second force was classification or planned space for patients in different stages of illness.

A third and powerful force was promotion and protection, which to some extent was helped by space and classification. Patients deeply depressed, confused, and deluded were placed in wards and rooms where they need take no responsibility for themselves; as their confusion cleared they were placed where they assumed small responsibilities; another promotion to the convalescent ward led to a final promotion to home and to all responsibilities. A patient on a long trip to a psychiatric hospital in which she had been a patient years before desperately attempted suicide again and again; on going through the

hospital gates she suddenly relaxed and said "Now I don't need to do anything—the responsibility is the hospital's!" She wanted protection, the other side of promotion—protection which was to be taken away as she became ready to take responsibility. The more promotion the less protection.

A fourth therapeutic force was occupational therapy with recreation, music, lectures, libraries, and school classes. Dr. Galt of Virginia in 1846 wrote a book in which all of these methods were explored and described in operations in the different hospitals. The force of occupational therapy was stronger in 1850 than it is now, tremendous advance in techniques being nullified by "too many patients."

A fifth force then as now was the removal of a patient from the complications of life at home, where his disturbed conduct created widespread reverberations to a place that took his unusual behavior and remarks calmly and understandingly with tolerance—and 100 years ago that place was better heated and ventilated than his home and offered more entertainment.

There is a sixth and last therapeutic force that is expressed in words somewhat different from our own. Then there was deliberate psychotherapy which was called "moral treatment." A sentence from Isaac Ray comes closest to present-day terminology:

The good superintendent . . . carefully studies the mental movements of his patients. He never begrudges the moments spent in quiet familiar discourse with them for thereby he gaineth many glimpses of *their inner life* that may help him in his treatment.

John Butler spoke for "individualized treatment." Galt spoke for treating a mental patient "as every honorable, well-bred man treats another in the common intercourse of society." "We meet patients as companions and equals." Dr. Woodward accepted patients' pledges to return from a visit or from shopping: "give them your confidence; respect them and they will respect themselves." Dr. Kirkbride accepted suggestions from a patient returning for a visit. The Friend's Hospital had a Restorative Society composed of patients who elected their own officers and

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ran their own meetings. Patients from other hospitals published their own newspapers or magazines. Inside the hospitals there was a general feeling that mental patients could be cured—in the enthusiasm the claims of cures were carried too far. In the old days doctors did not have the cold, objective poise in the presence of new ideas that we all possess now! But they had psychotherapy more based on kindness than is ours and directed to and influencing patients who were far less sophisticated than those we know now.

Here is a statement from the *American Journal of Insanity* of 1857: Any institutions where patients are "subjected to a machine-like, monotonous daily routine, denied the greatest freedom that their mental states will allow, and denied all the occupation and amusement they need, falls far behind the spirit of the age." Such an institution was behind the times 100 years ago!

At present, and in the near future, it is and will be more difficult to apply the old therapeutic forces than to use modern discoveries, and yet the new depend on the old. If we get rid of overcrowding, furnish adequate personnel, make it possible to show kindness and respect for each individual patient, we have a basis on which to apply new methods of therapy—new electrical, chemical, or psychotherapeutic methods—with effectiveness and discrimination and with proper records and controls. The old therapeutic forces can make modern therapies effective.

A practical point is this: the public and the legislatures should never be allowed to feel that promoting the promising new forces relieves them from the obligation to furnish to all psychiatric hospitals the therapeutic forces that were in common use in the year 1856.

As conclusion I read what Charles Dickens said about the Boston State Hospital in his *American Notes*, dated 1842.

It is admirably conducted on those enlightened principles of conciliation and kindness. A thorough confidence [is] established between the physicians and the patients in respect to the nature and extent of their hallucinations—In the labor department every patient is trusted with the tools of his trade. For amusement they walk, run, fish, paint, read and ride out to take the air in carriages provided for the purpose. They have among themselves a sewing society to make clothes for the poor, which holds meetings, passes resolutions—and conducts all

its proceedings with the utmost decorum. Once a week they have a ball, [at which time] immense politeness and good breeding are observed throughout.

It is obvious that one great feature of this system is the inculcation and encouragement, even among such unhappy persons, of a decent self-respect.

And in the same year my predecessor, Dr. Kirkbride, wrote these fine sentences:

The value of employment in treatment is now so universally conceded that no arguments are required in this favor. Its value cannot be estimated in dollars and cents. . . . The object is to restore mental health and tranquillize the restlessness and mitigate the sorrows of disease.

## DISCUSSION

LUCY D. OZARIN, M. D. (Washington, D. C.)—How the therapeutic forces were actually applied 100 years ago, I have had an opportunity of learning through the kindness of Dr. J. Butler Tompkins, Superintendent of the Brattleboro, Vermont, Retreat, who lent me a volume of newspapers issued by the patients of the Retreat from 1842-1846. The 4-page *Asylum Journal*, as it was named, was issued weekly for 2 years, and then monthly. Arrangements were made by the patients to exchange their *Journal* with more than 200 newspapers and periodicals in the United States. The *Journal* was also sent to postmasters with the request that it be given to families of insane persons, or other interested people. This is a remarkable example of mental hygiene education for the public.

The *Journal* provides excellent source material reflecting treatment in psychiatric hospitals at that time. Not only were the annual reports of the Brattleboro Retreat published, but also the annual reports of most of then-existing mental hospitals, as well as legislative actions pertaining to the mentally ill. Every issue contained stories or topics relating to psychiatry and mental illness, current news, and humorous items.

There were reports of current theories and treatments by such authorities as Esquirol and Isaac Ray, Dr. Bell, Dr. Galt, and others of the APA Founding Fathers. There were also contributions by clergymen and lay people and patients on their conceptions of mental illness. Many articles reflected the therapeutic principles Dr. Bond has outlined: adequate and comfortable living space, humane, kindly care that expressed respect for the patient, employment and progressive responsibility for patients, the use of patients to help other patients.

There are many articles in the *Asylum Journal* which state that mental illness is due to a combination of physical and mental causes. Hence moral treatment alone is not enough but medical remedies must also be used. This is an affirmation, if in the reverse, of Dr. Bond's important statement that, despite the promising new forces, we must continue to furnish to patients the therapeutic forces that have stood up for 100 years.

## ON GOING BERSERK: A NEUROCHEMICAL INQUIRY<sup>1</sup>

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Berserk was a mighty hero in Norse mythology. Legend states that he was the grandson of the mythical 8-handed Starkad-der. He was renowned for his consummate bravery and for the fury of his attack in battle. He had 12 sons who were his equal in courage. He never fought in armor but in his *ber sark*, which means "bearskin" in the Nordic languages. Thus the term berserk became synonymous with reckless courage. During the Saga Time(1) in Iceland and in the Scandinavian countries (870-1030 A.D.), and for some time prior to that period of careful historical recording, the Berserks, bearing the same name as the legendary warrior, arose as a predatory group of brawlers and killers who disrupted the peace of the Viking community repeatedly. Today in the United States we would probably use such slang terms as "mobsters" and "hood-lums" in classifying them.

There is a fascinating theory that *Berserksgang*, or the act of "going berserk," which was the hall-mark of their discordant behavior, may not have been a psychogenically determined habit pattern, but may rather have been due to the eating of toxic mushrooms. This idea, fantastic though it may appear at first glance, has won general acceptance among Scandinavian scholars according to Larsen(2). It is the purpose of this communication to review this theory in the light of present-day studies on hallucinogenic drugs which have chemical similarities with mescaline and LSD-25 (lysergic acid diethylamide) and which are capable of producing model psychoses.

Certain members of the vast botanical family of mushrooms have been among the most widely used "phantastica" throughout history to produce temporary psychoses. Among these are the *Amanita muscaria* and *A. pantherina* of Eurasia. The *muscaria*

species<sup>3</sup> is the more common. These are distinct from those species of the genus *Amanita* which kill the eater (*phalloides*, *verna* and *virosa*), and which contain a deadly hemolysin(3).

The *muscaria* fungus is commonly known as fly agaric. Albertus Magnus(4) noted before 1256 A.D. that when it was freshly cut and placed in a dish of milk or water it killed flies (*muscae*) when they ingested its juice, and Linnaeus reported that it had been advocated for killing bedbugs(5). It has long been used orgiastically by Siberian tribes of the Kamchatka peninsula. The first European to describe the practice was von Strahlenberg(6) in 1730. Prodigious feats of physical strength are reported under its influence. Vanderlip(7) wrote, "Curiously enough, after recovering from one of these debauches, they claim that all the antics performed were by command of the mushroom." The myths of the Koryaks contain the belief which is held to this day that a person affected with fly agaric is guided by the spirits of the Wapaq which live in the mushroom. If an old man would eat agaric and the Wapaq within the agaric should whisper, "you have just been born," the old man would begin to cry like a baby. If the Wapaq should say, "go to the afterworld," then the old man would die, according to their belief(8).

Jochelsen(9), who travelled among the Koryaks in 1900-01, wrote:

Fly agaric produces intoxication, hallucinations, and delirium. Light forms of intoxication are accompanied by a certain degree of animation and some spontaneity of movements. Many shamans, previous to their seances, eat fly agaric to get into ecstatic states. . . . Under strong intoxication the senses become deranged; surrounding objects appear either very large or very small, hallucinations set in, as do spontaneous movements and convulsions. So far as I could observe, attacks of great animation alternate with moments of deep depression. The person intoxicated by fly agaric sits quietly rocking from side to side, even taking part in conversations with

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<sup>3</sup> This beautiful fungus crowned by a vivid red cap dotted with white spots was depicted by Walt Disney in the "Dance of the Mushrooms" with Tchaikowsky's *Nutcracker Suite* music in *Fantasia*.

his family. Suddenly his eyes dilate, he begins to gesticulate convulsively, converses with persons whom he imagines he sees, sings, and dances. Then an interval of rest sets in again. However, to keep up the intoxication additional doses of fungi are necessary. . . . There is reason to think that the effect of fly agaric would be stronger were not its alkaloid quickly taken out of the organism with the urine. The Koryak knows this by experience, and the urine of persons intoxicated with fly agaric is not wasted. The drunkard himself drinks it to prolong his hallucinations, or he offers it to others as a treat.

The drinking of the Siberian mushroom eater's urine during these amanita debauches has been noted by many other travellers (10). An early satirical comment on this practice was made by Oliver Goldsmith (11) in 1762. It is reported that such urine can be drunk successively by as many as 5 people, passing in and out of one into the next, so that all of them can gain the hallucinogenic effect from one dose of mushrooms. It would be most revealing to know the chemical composition of the hallucinogen which is passed around in the urine of these tribesmen, but this fact is not known. Studies looking toward the solution of this problem have been undertaken by Evan Horning and his associates at the National Institutes of Health, Bethesda, Maryland. In 1953 Wieland and Motzel (12), employing paper chromatographic analysis, determined that this mushroom, *Amanita muscaria*, as well as *A. mappa*, and *A. pantherina*, all contained bufotenine, or n-n-dimethyl serotonin, an indole compound first isolated and defined chemically by Handovsky (13) in the skin of poisonous toads.

I am indebted to Dr. Arthur Drew (14) of the Department of Neurology of the University of Michigan for his account of a modern version of *A. muscaria* poisoning.

Dr. Drew reports that the patient, a middle-aged tavern keeper, picked some wild mushrooms and ate them at 10 o'clock one night in October 1955. These mushrooms were later identified by the botany department as *A. muscaria*. Two hours after ingestion the patient had an explosive onset of diarrhea, profuse sweating, excessive salivation and vertigo. He fell asleep and wakened at 2 a.m. completely disoriented, irrational, and violent. On admission to University Hospital, Ann Arbor, Michigan, his nail beds were dusky and he appeared cyanotic, B.P. was 150/90, pulse 120, respirations 24/min., and the temperature 98.4°. He did not react to deep pain stimulation, but responded to pinprick.

He was disoriented in all 3 spheres. Somnolence alternated with periods of excitement. He thought that he was in hell and identified the interne, nurses, and attending physicians as Christ, Satan, God, or angels. Nursing notes on admission indicate that he was threshing about in bed, talking constantly and irrationally.

As the day wore on the content of his hallucinatory and illusional output remained almost entirely religious. He constantly misidentified a tall resident physician as Christ. He kept referring to nurses and other attendants as God or angels. He felt that he was in the Garden of Eden, and then in hell. As evening came he cleared up mentally, lost his motor excitement, and felt relaxed. All laboratory tests were within normal limits. He appeared to be recovered on the following morning and was discharged.

Another mushroom eating practice deserves mention. Ever since the time of the conquest of Mexico by the Spaniards in 1522 there have been references (15) to a sacred fungus, *teonanacatl*, employed by the Aztecs and other Mexican Indians. Schultes (16) identified this as the *Panaeolus campanulatus*, var. *sphinctrinus* mushroom in 1938. This "fungus of the devil," as the early Spanish priests called it, is now under study by Mr. R. Gordon Wasson (17), and his wife, Dr. Valentina P. Wasson, of New York City. The Wassons have eaten the mushrooms and report visual hallucinations in brilliant colors, an ecstatic state of heightened perception, loss of time and space perception and a serene feeling of inward peace while being drawn into an "other-worldly detachment" during dissociation periods of at least 6 hours duration.

Mr. Wasson reports that there are many colloquial names for the mushrooms at the present time. He writes:

Every Indian language in Mexico has its own name for the divine mushrooms, and, as you would expect with sacred growths, every village is apt to use evasive or euphemistic names as well. In Nahuatl (the language of the Aztecs) the name was *teonanacatl*, 'God's flesh,' but we find in a village where classic Nahuatl is still used today that the word is *apipiltzin*, "little children of the waters."

He has also noted the terms *nti-si-tho*, *tu-m'uuh*, *mbeyo'*, etc., used by other ethnic groups. He has identified species of 4 genera of Mexican mushrooms in use at this time for their hallucinatory properties—*panaeolus*, *stropharia*, *conocybe*, and *drosophila* (also called *psathyrella*). Species identification

among the genera has not been completed. The chemistry of these mushrooms is not known. It would be interesting to determine whether or not they contain bufotenine. There is no report of urine drinking among Mexican ritual mushroom eaters.

Other evidence suggests the use of bufotenine as an hallucinogenic substance. Fra Ramon Pane, who came to America with Columbus on his second voyage, described the ceremonial use of cohoba (18), a snuff derived from the seeds of the *piptadenia peregrina* tree, as early as 1496, reporting that "it intoxicates them to such an extent that when they are under its influence they know not what they do." This inhalant was so potent in producing temporary dissociation states that it became tribal custom for the women to tie up the Otomaco Indians when they used it. Stromberg (19) determined that bufotenine was the active indolic principle of cohoba. Recently Horning *et al.* (20) were able to find a few milligrams of the snuff in the bottom of a Piaroa ceremonial snuff box in the Smithsonian Institution. Paper chromatographic analysis showed that this snuff contained large quantities of bufotenine. Evarts (21) and Fabing (22) have also found that the effect of bufotenine in experimental animals is reminiscent of LSD-25. It would appear, then, that there is chemical, experimental, and anthropologic evidence that bufotenine is one of the hallucinogenic indoles, that it is distributed widely in nature, and that one of its chief sources of supply is the mushroom.

Turning to the Viking hoodlums, a vivid description of their behavior is given by Schübler (23), who relies on the renowned Norse historian, Munch, in his account:

In the old Norwegian historical writings it is mentioned, in many places, that in olden times there was a specific kind of giants who were called *Berserks*, that is, men who at certain times were seized by a wild fury, which, at the moment, doubled their strength and made them insensible to bodily pain, but which also deadened their humanity and reason, and made them like wild animals. This fury, which was called "Berserksgang," occurred not only in the heat of battle, but also during laborious work. Men who were thus seized performed things which otherwise seemed impossible for human power. This condition is said to have begun with shivering, chattering of the teeth, and chill in the body, and then the face swelled and changed its color. With this was connected a great

hotheadedness, which at last went over into a great rage, under which they howled as wild animals, bit the edge of their shields, and cut down everything they met, without discriminating between friend or foe. When this condition ceased, a great dulling of the mind and feebleness followed, which could last for one or several days (24).

One of the curious aspects of *Berserksgang* is that it disappeared abruptly in the twelfth century A.D., after plaguing Viking socio-political life for more than 3 centuries. In 1784, Samuel Lorenzo Ødman (25), a theologian at the University of Upsala, undertook to explain the phenomenon of *going berserk*. He reviewed the Sagas for descriptions of the state. He found King Halfdan's Berserks depicted in Rolf's Saga in this manner:

On these giants fell sometimes such a fury that they could not control themselves, but killed men or cattle, whatever came in their way and did not take care of itself. While this fury lasted they were afraid of nothing, but when it left them they were so powerless that they did not have half of their strength, and were as feeble as if they just came out of bed from a sickness. This fury lasted about one day.

Ødman also recounted a tale from the Hervarar Saga. There were 12 brothers who lived on the island of Samsøe in Denmark. Orvar-Odd sailed to the island with his Viking ships, debarked and went inland to visit his cousin, Hjalmar. The 12 Danish brothers went berserk and killed his crews to a man. When he returned to the shore with his cousin, they encountered the Berserks who were now in the enfeebled state after their fury. Hjalmar killed one and Orvar-Odd killed the other 11. Ødman saw in this fury followed by a state of exhaustion a paroxysm, and stated:

I am not of the opinion that these ecstasies can be explained as effects of a peculiar temperament or of auto-suggestion because . . . they were not able to keep up their hated arrogance between paroxysms.

He argued further:

Since the vegetable kingdom gives us various means to bring our power of imagination into chaos and to induce the most ferocious excesses of courage, I am inclined to believe that the Berserks had knowledge about such an intoxicating means, and that they made use of it and kept it secret so that their prestige would not be reduced by the general populace's knowledge of the simplicity of the technique.

He then reviewed the possible botanical products indigenous to Scandinavia which might have been used in this manner, and decided that "flugswamp," the *Amanita muscaria* mushroom, was the one which solved the riddle of the Berserks.

He then compared the accounts of *Berserksgang* in the Sagas to the "amanita debauches" of the Koryaks and other far-Eastern Siberians, and found them to be almost identical behavior patterns. In furtherance of his contention he turned to theological history. He stated:

What in particular seems to me to argue for flugswamp is the fact that to partake of it is a custom from that part of Asia from which the pagan god Odin, with his pantheon, made their migration to our North. . . . Its [the mushroom's] use was spread by these hordes who used them and travelled northward. The history of the Berserks in our North begins with Odin's coming. Not only this, but it fits so well with the intentions of a conqueror who . . . could make himself feared and safe among foreign people.

More than a century later, in 1885, F. C. Schübeler, a physician and Norway's great botanist, arrived at the same conclusion. He wrote (26):

I still have a vivid remembrance that, on confronting all the symptoms that appeared under the so-called "Berserksgang," I came to the conviction that this paroxysm hardly could be anything but a kind of intoxication, the symptoms of which reminded me of the effects of taking fluesop\*. . . . Some time after I had come so far in this matter, I happened to find, while looking for something else, that the Swedish professor, Samuel Ødman had uttered the same opinion a hundred years ago.

Schübeler agreed with Ødman concerning the similarity between *Berserksgang* and the behavior of Siberian mushroom-eaters. He added that he could not agree with Munch that the condition was a "periodically returning insanity," because the symptoms were of a peculiar sort not ordinarily seen by physicians, and because they were the same each time a man went berserk. He argued that the effects could not be those of malted beverages because they produced quite different behavior patterns, and that distilled spirits were not known in Norway before 1531 A.D. In addition, opium and cannabis were unknown to the Nordics in Saga times.

\* Norwegian equivalent for the Swedish "flugswamp."

He argued too, in favor of secrecy concerning mushroom eating among the Berserks, and that for this reason no accounts of the practice were written down. He wrote:

The Berserks were feared by everyone, and they could, in a certain sense, enforce their pleasure. It is therefore in the nature of the case that they tried as best they could to keep this peculiar reputation in the eyes of the people. Hence, the knowledge of the intoxicant was probably transferred as a secret from individual to individual.

It is worthy of note that the Wassons find a similar attitude of secrecy in the Mexican peons who use sacred mushrooms today.

Schübeler pointed out, too, that the more enlightened Viking leaders soon learned that the state was one which could be prevented, and therefore could be legislated against. He wrote:

Before Erik Jarl left Norway he called together (in 1015 A.D.) the feudatories and the mightiest peasants in order to deliberate with them about the lawgiving and the rule of the country. At this meeting camp-fighting (holmgang) was abolished, and Berserks and robbers were outlawed. In Thorlak's and Ketil's Icelandic Christian Law, which was adopted in 1123 A.D., there is the following decree: "If someone goes berserk, he is punished with three years of banishment (fjorbaugsgard), and the men who are present are also banished if they do not bind him; but if they bind him, none are punished. If this is repeated, then the punishment occurs."

Schübeler regards this as proof that the Vikings came to know that the paroxysm was temporary and preventable. *Berserksgang* ceased after this law was passed.

Fredrik Grön (27) reviewed the subject of the fury of the Berserks in 1929, and did not agree with the Ødman-Schübeler hypothesis. He points out that because of the general reliability of the historical Icelandic family sagas, none of the authors who have written about *Berserksgang* have raised any doubt about the reality of the phenomenon, but the explanations for it have varied over a wide range. Even in the more fantastic prehistoric sagas there are enough common traits in their descriptions to establish a background of reality for the phenomenon. He feels, however, that the best explanation would be that of ecstatic fury psychogenically determined in a group of aggressive psychopathic personalities.

Recently we (28) have had the opportunity

to study the effects of the intravenous injection of bufotenine\* in the human during the course of our inquiries into possible chemical factors in the causation of schizophrenia(29). The subjects were healthy young long-term convicts at the Ohio State Penitentiary(30). All were well above the normal intelligence level, all had been college students, none were recidivist criminals, and all were considered to be relatively stable emotionally. Injections of one part bufotenine base in 1.8 parts creatinine sulfate were given steadily over a 3-minute period. A dose of 1.0 mg. bufotenine produced only a sensation of tightness in the chest and paraesthesiae of the face. Two mg. produced a "tightness in the stomach" plus flushing and a purplish hue of the skin of the face. Four mg. produced a sensation of tingling in the face and neck, a sense of chest oppression, a subjective report that "a load is pressing down from above and my body feels heavy," and a "very pleasant Martini feeling." This was followed by a visual hallucinations of vivid red and black blocks moving before the visual field, inability to concentrate, and a feeling of great placidity and less anxiety than before the onset of the experiment. The face appeared lividly purple for 13 minutes.

Eight mg. produced an immediate sensation of light-headedness, burning in the face, hyperpnea, deep purple facial color, and a sense of calm. At the end of the injection, the subject blurted, "I see white straight lines with a black background. I can't trace a pattern. Now there are red, green and yellow dots, like they were made out of fluorescent cloth, moving like blood cells through capillaries." Six minutes later he reported that he felt relaxed and languid. In retrospect he said, "Even at the height of this, my mind felt better and more pleasant than usual."

Sixteen mg. produced severe purpling of the face and facial sweating, tingling sensation throughout the body, a feeling that his chest was crushed, and the onset of hallucinations of purple spots on the floor, all in rapid succession before the injection was completed. Three minutes later the visual phenomena were gone, but space perception was

impaired. He complained of difficulty in concentration, and could not subtract serial 7's from 100, saying that he was "all loused up." During the next hour his face remained deeply purple, he was unable to express himself in words, stating that his mind felt crowded, and he showed motor restlessness, stating that he wanted to "walk it off" and that "my body feels nervous." Time and space perception were grossly impaired, and he expressed depersonalization feelings with such statements as "I am here and not here."

Nausea was an initial accompaniment of the syndrome as the injection began, and proceeded to retching in the 16 mg. dose. Nystagmus and mydriasis occurred in all cases, and increased in magnitude and duration as the dose increased. Pulse and blood pressure changes were minimal throughout. Another noteworthy finding was that of relaxed placidity and languor which all subjects reported for as much as 6 hours after injection. They lay contentedly in bed, feeling pleasantly relaxed, stating that they felt a lack of drive rather than a sense of fatigue.\* These observations were repeated on a later date(31) using 6 other convicts as subjects. The results were of similar type, except that lesser symptoms occurred if the injection was slowed to a 10-minute period rather than a 3-minute one.

It would appear then, that intravenous bufotenine is hallucinogenic for man, that its action is rapid and the duration of effects is fleeting except in higher dosages. As the dose increases distortion of time and space perception occurs, as does depersonalization and bodily restlessness. Mydriasis and nystagmus make one think that at least a portion of this drug's action is in the brainstem tegmentum. The picture which remains most vividly in the memory of the observers is the purple faces of these subjects. If the color of an eggplant were diluted, it would approximate the hue which they assumed. An artist could describe the color more accurately, but he would have to use the word "purple" in his description.

Students of model psychoses are confronted constantly with the general "schizo-

\* The drug was kindly supplied by the Research Department of the Upjohn Co., Kalamazoo, Mich.

\* E. L. Kropa and R. D. Morin of the Battelle Memorial Inst. kindly assisted in making these observations.

phrenic" response of subjects despite wide individual variations in behavioral activity. Much of this case-to-case variation may be due to personality differences or may be culturally determined. In many Orientals, for instance, the use of cannabis provokes erotic ideation and behavior for the most part, whereas in our culture it is used primarily by devotees of music who do not become sexually stimulated by the drug in most cases.

Within the dimensions of such variations there seems to be much in common between amanita mushroom-eating experiences and the response to intravenous bufotenine. When the purpling of the faces, as well as the hallucinations, the motor restlessness, the time and space distortion, the depersonalization, and the terminal languor of our experimental subjects are placed in juxtaposition with the accounts of the rage of the Berserks, the mushroom debauches of the Siberians, and Drew's recent case, the general similarity is striking.

It would appear, then, that recent observations on the human tend to support the Ødman-Schübeler hypothesis that the Norse giants ate the *Amanita muscaria* mushroom to produce the ecstatic reckless rage for which they are renowned, and which was a culturally accepted temporary psychotic aberration in their group. *Berserksgang* on a hill in Iceland in 955 A.D. might well have had the same neurochemical basis as an injection of bufotenine under experimental conditions in Columbus, Ohio, a thousand years later and three thousand miles away.

#### SUMMARY

The ingestion of hallucinogenic mushrooms by Siberian tribes of the Kamchatka peninsula and by Indians of the Mexican highlands has been carried out in ritual and orgy for centuries. Ødman and Schübeler have advanced the hypothesis that the furious rage of the Berserks in the heyday of Viking culture a thousand years ago was brought about by the same agency, specifically the *Amanita muscaria* mushroom. A few years ago it was found that these fungi contain bufotenine, or n-n-dimethyl serotonin, a substance which is under scrutiny at this time for its possible neurochemical role in the causation of schizophrenia. Recent observa-

tions on the intravenous injection of bufotenine in man disclose that it is an hallucinogen, and that its psychophysiological effects bear a resemblance to the *Berserksgang* of the Norsemen in the time of the Sagas. These observations appear to offer support to the Ødman-Schübeler contention that the famed fury of the Berserks was what we would call a model psychosis today.

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## SOME FACTORS IN THE EMOTIONAL REACTION OF CHILDREN TO DISASTER<sup>1,2</sup>

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### INTRODUCTION

On Saturday, December 5, 1953, at about 5:35 p.m., a tornado hit Vicksburg, Mississippi. It was over in about 10 minutes, but had caused considerable damage and loss of life. A motion picture theatre, filled with children attending a Saturday afternoon movie, was particularly affected. For this reason, the Committee on Disaster Studies of the National Research Council felt that an unusual opportunity existed to study selectively the effects of the disaster experience on children in the community. The National Institute of Mental Health was invited to participate in the study, and to supply professional personnel. One week after the tornado had struck, Bloch and Silber were in Vicksburg, where they spent 4 days interviewing parents, children, pediatricians, school officials, teachers, and community leaders. Arrangements were made at that time for co-operation to test, with a more rigorous design, certain specific hypotheses concerning emotional disturbance in children, as a response to the tornado. After the collection of the data, the third author, Perry, joined the project for the systematic study of the data.

This paper covers 2 kinds of observations. In the first section we present the design as conceived and carried out and the resulting findings. The second section describes certain over-all impressions growing out of the research experience.

<sup>1</sup> Read at the 111th annual meeting of The American Psychiatric Association, Atlantic City, N. J., May 9-13, 1955.

<sup>2</sup> This report is part of a study supported by the Committee on Disaster Studies of the National Research Council, National Academy of Science, and the National Institute of Mental Health. The report of the entire study will be published in the monograph series by the Committee on Disaster Studies: Perry, S. E.; Silber, E.; Bloch, D. A.: *The Child and his Parents in a Disaster*, Monograph #5, Committee on Disaster Studies.

<sup>3</sup> Child Research Branch, National Institute of Mental Health, Bethesda, Md.

### THE FORMAL STUDY

Because of the immediacy of the situation, the formal study was necessarily conceived and executed in haste and suffered from this fact. Two general areas of interest were investigated.

*What was the relationship between the child's emotional disturbance and the extent to which the child was actually involved in the disaster itself?*—(1) Spatial Involvement: Children could be placed in relation to their physical closeness to the tornado. Loose description would divide them into the group who were in the impact zone and those who were not. (2) Interpersonal Involvement: This could be described in terms of personal ties with other children or adults who were seriously involved or injured in the tornado. (3) A third group of children had neither spatial nor interpersonal involvement. (4) A fourth group of children had both spatial and interpersonal involvement.

*What was the relationship between the emotional disturbance of the child and the way in which parents handled the experience with him?*

It appeared obvious, clinically, that parental handling of the disaster experience influenced the ability of the child to deal with it. Some children appeared to have more difficulty, not only because of the experience undergone, but also because of the way their parents dealt with it. It seemed important to know more of the nature of those parent-child interactions which facilitated healthy integration on the part of the child. In order to narrow the field here, we decided to focus on the area of emotional responsiveness and communication between parent and child relevant to the disaster itself.

### DESIGN OF THE STUDY

*Selection of the Sample.*—One particular school in the community was selected for the

survey because it provided groups of children ranging from those who were seriously involved in the tornado to those who were not affected.

A questionnaire was distributed to all the parents of the children, together with a cover letter supplying information about the purpose of the survey. The questionnaires were returned to the school by the children. Approximately 520 questionnaires were thus distributed, 427 of which were executed and returned—better than 80%. The questionnaires mainly served the purpose of case-finding, by which families could be selected for interviews. On the basis of them, lists of respondents were constructed according to the criteria: (1) the kind of involvement events which the child had experienced in the tornado; and (2) whether or not the parents' comments on the questionnaire indicated that the child was emotionally disturbed (either before or after the tornado).

Questions answered by the parents as to whether or not some friend of the child or member of the family was killed or injured in the tornado provided one category, tentatively labelled "Interpersonal Involvement." If the child himself was injured or trapped by tornado wreckage or was generally in the damaged area, he fell into the category labelled "Spatial Involvement." With neither or both types of involvement, "Neither" or "Both" were the categories. Finally, the child was classed, on the basis of the questionnaire material, as suffering "Trouble" or "No Trouble." The questionnaires were thus separated into 8 groups.

*Contacting the Interviewees.*—Prospective interviewees were chosen by telephone contact from the names of the questionnaire respondents that appeared on the 8 lists of names. Very few of those contacted refused to be interviewed or cancelled their appointments. In this manner, 88 parents were interviewed, as were 3 families who had not returned a questionnaire, making a total of 91 interviews.

*The Interviews and the Reports.*—Each interviewer was furnished with a topical interview outline. The interviews were conducted in a relatively unstructured manner, allowing the interviewees to talk, for the most part, about whatever they wanted to, and

lasted from about three-quarters of an hour to 2 hours. The subjects discussed included: (1) the actual involvement or tornado experience events; (2) behavior changes or symptoms in the child since the tornado; (3) the way the parents responded to and handled the experience and the reaction of their children to it. General background information about family members and their relationships was obtained from most families—although this was not actually included in the interview schedule. Aside from the use of a common topical interview outline, no specific effort was made to achieve comparability in interview techniques, in information to be gathered, or in the way in which the interview would be reported.

*Methods Used for Analysis of Data.*—The goal in the analysis of the data was to organize them so they would meaningfully fall into the following categories: (1) tornado involvement events or experience undergone by the child; (2) the parents' way of handling the experience; (3) the presence or absence of symptom formation of emotional disturbance in the child; and (4) predisposition to emotional disturbance. The analysts' efforts to construct a set of subcategories that would allow the interview material to be coded showed that the individual items were too infrequent for this, nor did condensation of the material permit it.

We decided that the most effective means of analysis was the exploratory impressionistic overview of the interviews on a survey basis. Each interview was read by both psychiatrists, and immediately discussed on a free-associational basis, with the conclusions and certain pertinent facts noted. After about a third of the interviews were so analyzed, and a general consensus had grown up between the 2 psychiatrists, the remaining interviews were divided and done individually by one or the other, together with the project analyst who posed the questions for each interview and recorded the ratings and observations of the psychiatrist. The main conclusions presented in the next section of this report are drawn from this procedure. Certain quantitative measures were used. They do not yield uncontaminated descriptions of each variable singled out—that is, the measurement of one variable is, to some extent,

influenced by measurements of other variables—and so any correlations or significant associations between variables may be somewhat tautological.

After the 2 psychiatrists had completed their assessments of the individual interviews, a greatly scaled-down content analysis code was used to describe certain items from the interviews so that they might be related to the psychiatrists' assessments. This content material and the psychiatrists' assessments of the emotional disturbance of the family and the individual child were entered on IBM cards for cross comparisons.

*Conclusions on the Basic Questions Asked.*—The 88 families selected for interviews had a total of 120 of the 427 children on whom we had questionnaires. A child for whom we had an individual questionnaire, we here call a questionnaire-child. Fifty-nine of the families interviewed had 1 questionnaire-child; 26 had 2 each; and 3 had 3 each. In addition were the 3 other families, who had not filled out questionnaires, with 1 child each.

Since the selection of families for interviewing was in the main based upon our classification of their children's questionnaires, it is pertinent to see how different children in the same family were classified. There were 12 families with 2 or 3 questionnaire-children all classified identically. However, there were 17 such families in which the children were classified differently. For example, one child might have been classified as "None-No Trouble" while his sibling was classified as "None-Trouble" or, say, "Spatial-No Trouble." In other words, there was a sizable number of multiple-child families in which the children had different experiences or reactions, according to the classification of the questionnaires. In the final analysis, we had a total of 185 children on whom we had information and on whom we chose to focus our attention. Such children will hereafter be termed interview-children to distinguish them from the less inclusive group of 120 questionnaire-children. The age distribution of the interview-children is shown in Table 1.

#### EMOTIONAL DISTURBANCE

The 2 psychiatrists' ratings of the children are as follows: 113, no emotional disturb-

TABLE 1  
INTERVIEW-CHILDREN IN TERMS OF AGE

Age Years	Number
Infancy—	
2	19
3	12
4	10
5	10
6	27
7	20
8	20
9	16
10	18
11	15
12	16
13	1
14	0
15	1
Total	185

ance; 32, mild; 24, severe; and 16, insufficient data to make a judgment on this question. As used here, emotional disturbance loosely refers to the presence of overt anxiety, anxiety equivalents, symptom formation or intensification of pathological character traits.

Typically, there was an increase in dependency needs manifested by behavior which appeared to aim at reestablishing situations of earlier security; for example, a tendency to cling to parents and to remain closer to them, a need to stay near the home, asking to sleep with the parents, and so on. Other kinds of regressive behavior such as enuresis and the abandonment of previously learned skills were described. There were other symptoms which seemed to refer more directly to the disaster itself—for example, night terrors during which the experience would be relived. Tornado games appeared in some children's play. There was also a tendency toward general irritability and sensitivity to noise on the part of some children. Finally, there were phobic and avoidance symptoms connected with experiences marginally related to the tornado. Even going to an open air movie was avoided by those who had been in the theatre during the storm.

In the following discussion, we examine only those cases on which we have information as to their emotional disturbance and as to the other variable being considered. This means that the totals in any disturbance cate-

gory may be less than the original totals of disturbed and nondisturbed.

#### VARIABLES RELATED TO EMOTIONAL DISTURBANCE

*Involvement in the Disaster.*—The particular kinds of disaster experiences we have called involvement events. The question was: do any of these events offer clues for distinguishing which children were later disturbed? We did not collect samples of children who had certain types of experience, so as to see how many of them became disturbed. We did compare the disturbed in relation to the group of undisturbed children to see whether any relationships existed between specific kinds of involvement events and emotional disturbance. Five such relationships appeared, and one other, which we had expected to, did not.

*Aware of the Tornado.*—Being aware of the tornado at the time that it happened and being later emotionally disturbed were significantly related. ( $P > .05$ ).<sup>4</sup> Being aware of the tornado is a gross variable and might be expected to be a kind of prerequisite for most other involvement events that could be considered more traumatic, such as, being personally injured by the tornado. (That such a simple characterization of a child's disaster experience is significantly related to emotional disturbance is probably a function of its association with much more traumatic involvement events.) We found that 87% (41 of 47) of the emotionally disturbed children had been aware of the tornado at the time that it happened; whereas, only 69% (67 out of 97) of the undisturbed children had been thus aware.

*Presence in the Impact Zone.*—Being emotionally disturbed and having been in the impact zone were significantly associated ( $P > .01$ ). Here, however, the significant association was found for those children who were severely disturbed, a total of 61% of whom had been in the impact zone. On the other hand, the mildly disturbed were more like the undisturbed in this characteristic: Only 32% were in the impact zone.

<sup>4</sup> Probabilities of significant associations (except where otherwise noted) were determined by the chi square test, and they will be reported as better than, for example, .01 ( $P > .01$ ) or considerably better ( $P \gg .01$ ).

*Vicarious Involvement through the Experiences of Friends.*—Although the child himself may not have experienced certain involvement events, we hypothesized that he might nevertheless be disturbed if his friends had been killed or injured. In other words, the child's identification with his friends might result in his being disturbed by what happened to them, for what happened to them would be a direct experience for him. Interestingly enough, we did not find a significant relationship between children's emotional disturbance and their vicarious involvement through the injury and death of friends.

*Personal Injury.*<sup>5</sup>—Eight of the children studied had been personally injured, 7 severely, according to our criteria, the eighth having suffered only a cut finger. The 7 severely injured were disturbed, accounting for one-eighth of all the disturbed children. The child with the cut finger was one of 113 undisturbed children. The relationship between injury and disturbance was found to hold statistically with this small sample.

*Vicarious Involvement through the Experience of Family Members.*—As in the case of vicarious involvement through friends' experiences, the death or injury of family members was considered as an involvement event. Twelve of the children had immediate family members killed or injured. Eleven of these were disturbed and accounted for somewhat less than one-fourth of all the disturbed children. Of the twelfth child, the interviewer noted that she could not get the respondent-mother to talk much about the child, for, despite repeated starts by the interviewer, the mother would always return to discussing the child's older sibling, who had been injured in the tornado. What information the interviewer did obtain resulted in a "no disturbance" rating by the psychiatrists. Fisher's exact test, applied here, indicated a significant relationship between emotional disturbance and the vicarious in-

<sup>5</sup> For examining the relationship of this and the following 2 variables to emotional disturbance, we used Fisher's exact test. The hypothesis of independence was tested and rejected in all 3 instances ( $P > .01$  or  $\gg .01$ ), indicating a significant association in each instance between the involvement event and emotional disturbance.

volvement through injury or death of family members.

*Dissociative-Demanding Parental Response.*—Another specific experience considered as an involvement event was the child's experience of his parents' immediate response to the tornado, that is, the way the parent reacted in the presence of the child. The central aspect of the immediate reaction is its meaning for the child in terms of offering him a ready-built perception or structure of the event—a cue from which to determine his own actions and feelings in the situation. Although we considered this as an involvement event, it was also the closest approximation we could achieve of a measure of differential parental handling of the disaster experience *vis-a-vis* the child.

One pattern of parental response consisted of a dissociative or confusional state, accompanied by an indirect or direct plea for the child to assume the supportive and protective role. This we have called the dissociative-demanding response. In these cases, parents reported that they themselves "went to pieces," fainted, or asked for help from the child rather than giving aid or support to him. In analyzing the data, the parental reactions were dichotomized into those in which we had information that one or both parents assumed such a dependent role, and all other cases. Eight of these children accounted for about one-sixth of the total of disturbed children. The other child was not disturbed. She was the one mentioned above about whom the mother could not be induced to talk. A significant association was established between the child's experience of his parents' dissociative-demanding reaction and his emotional disturbance.

*Age and Sex.*—The only significant and testable age differential in terms of disturbance was that between school children (6-12) and preschool children (below 6). School-age children were more apt to be disturbed than preschool children ( $P > .05$ ), the percentages of the total of disturbed children being 88 and 12 respectively. For the undisturbed, the respective percentages were 73 and 27. No preschool child was rated as severely disturbed. We found no relationship between the sex of the child and disturbance; 50% of the disturbed children were

males and 50% females. Of the undisturbed children, 57% were males; 43% females.

*Parents' Psychopathology.*—A family characteristic which we were able to draw out of the information available to us—although this was not originally planned for—was the presence or absence of a *reported* history of some psychopathology in one or both parents. That is, we were able to dichotomously classify parents as having no history of psychopathology or as having a history of some chronic psychiatric or psychosomatic symptom, a nervous breakdown, or chronic poor relationships with some member of the (immediate or extended) family.

Unfortunately there was not enough information on a large number of parents, so that in the end our sample amounted to the parents of 87 children, 57 of whom were undisturbed and, 30 disturbed. This means that the analysis of these cases was based on a sample biased in the direction of containing proportionately more disturbed children than the total original sample of 169 interview-children. However, within the limits of the sample, a statistically significant relationship was observed which corresponds to what Carey-Trefzger<sup>6</sup> found in her studies of emotionally disturbed English children who underwent war trauma during the time of the London blitz, *i.e.*, a relationship between a history of parental psychopathology and the child's emotional disturbance after a disaster ( $P \gg .01$ ).

#### OVER-ALL IMPRESSIONS

In this section, we discuss certain impressions derived from on-the-spot experience with the disaster situation and from exploration of possible meanings latent in the interview data but *not* revealed by statistical treatment.

*Tendency to Deal with Disaster Experience by Denial and Suppression.*—It was apparent in our talks with parents and community leaders that a common mechanism of defense utilized by many persons was to attempt to suppress any meaningful recollection of the disaster. Many had difficulty in answering questions children would ask about

<sup>6</sup> Carey-Trefzger, Charlotte J. J. Ment. Sci., 95: 535, 1949.

the tornado, especially when they dealt with the death of other children or people who were close to the children. Many persons felt that it was best to get the children back into the kinds of activities that they had been doing as quickly as possible, but without acknowledging that anything had really happened.

Some parents seemed to become so anxious in recalling the experience that the only way they felt they could deal with this anxiety was by a suppression or by attempting to deny that the experience was really as bad as it actually was. For this reason, it was difficult for them to tolerate discussion with their own children. Since some parents indicated, in this way, that one could not openly discuss the experience, the disaster may have been made even more frightening for their children. In our experience, when the parents became freer in dealing with it, recognizing the great relief in being able to discuss it openly and frankly, the whole communication process in the family was facilitated. It became easier for them to tolerate the kinds of questions children asked, and the anxieties they faced. Thus the child, too, could participate in the entire family's reintegration, communicating openly with his parents about his own feelings, fears, apprehensions, questions, and uncertainties. The family attitude thus shifts from one of trying to forget about the experience to one of learning to live with it.

*The Need to Deal with the Problems of Parents Before Approaching the Child.*—One could not approach parents for information about their children unless one was prepared to deal with the parents' problems first. If one does not recognize this, interviewing may create so much anxiety in the parents as to result in a barrier to any type of investigation relative to understanding children. This is important for investigators who may in future go into communities where children have been involved in a disaster.

*Use of Parent Groups.*—From our experience in setting up a group meeting with some of the parents who had been most seriously affected in the disaster, we believe that such meetings can be used very effectively for diagnostic and therapeutic purposes with large numbers of parents. They could be set up with a limited goal, the aim being

simply to provide an opportunity for parents to meet and help one another through a discussion of their mutual problems. The group setting seems to be especially helpful in dealing with the defenses of denial and suppression.

*The Family as a Unit and the Appropriation of Roles.*—While we used the individual child as the focus of attention, perhaps a more meaningful conceptualization might have focussed on the family as a relatively discrete system, within which each child could be thought of as an inseparable part. We might have better understood our subjects if we had thought of them in terms of their performing certain functions within a more-or-less autonomous whole—the individual family system. For example, in families with more than one child, it often seemed as if one of the children had appropriated a certain role, such as being "disturbed." This might mean that there was no more room within the family for someone else to perform the same role.

*Reaching Out for Other People as a Reintegrative Operation.*—Many investigators have commented on the reassurance that people experience by ascertaining the safety of family members and friends immediately after a disaster occurs. The same sort of reassurance has been noted in helping in rescue and other sorts of postdisaster help for persons unknown to the individual. We found it helpful to think about this as a process of reintegration in terms of reaching out for others. Psychodynamically it is related to the mutual regressive patterns described earlier. As such, it could be seen in many other forms and for longer periods than those just mentioned. Younger children, as we have mentioned, usually wanted to remain close to their parents; and parents wanted to keep their children close about them—for many days or weeks after the tornado. Adolescents found reassurance in reaching out to others via working in canteens and in other rehabilitation work, in denying themselves certain pleasures so that benefits could go to others more needful, and so forth. Adults temporarily broke down certain of the reservations between Negro and white—even among persons who had

never known each other before. For example, one upper middle-class family took in a strange Negro child to sleep in the same bedroom as the family's own child. The white child wanted to sleep with an adult member of the family, and so did the Negro child. The white child's request was granted and, although the Negro child was not put in the same bed with the adult, a cot was drawn up next to the bed as a partial acceptance of the child's request. It was not uncommon for respondents to comment about Negro and white working side by side during the emergency period.

It is these sorts of things that we would lump together as representing some kind of need for belongingness or reaching out for others, which seemed to be a typical and pervading disaster response. We are inclined, also, to hypothesize that the hostile "snap-back" of disaster area populations which has often been observed—that is, the tendency after some weeks have elapsed for the population to be critical of themselves and of any relief agencies, as opposed to earlier more favorable attitudes—is related to a need to re-establish distance between the self and others after the reaching-out period.

*Mental Health Studies and the Entry Provided by the Disaster.*—One of the more

difficult problems that confronts the public health study of mental illness and health is the assessment of some sort of base-line of mental health within a community. It may be extremely difficult to obtain the cooperation of a cross-section of a population necessary to study the community mental health level, because of the problem of interviewing people about rather sensitive matters when such interviews are for purely informational purposes, rather than as an immediate source of help to the interviewee. Certainly this difficulty exists in disaster interviewing. However, our respondents were as a whole very cooperative in discussing material that ordinarily is not so readily accessible to the research interviewer. This was, we think, in large part due to the peculiar circumstances of a disaster. That is, people are probably more accessible, from the standpoint of *rapport*, during the immediate post-disaster period. At any rate, they may be more ready to talk about parent-child difficulties, psychological symptoms, family problems, and so forth, when such interviews are initiated on the rationale of studying responses to disaster. Disaster research, therefore, provides an entry to the study of certain subjects not so readily accessible in other circumstances.

## STRESS AND PSYCHIATRY<sup>1, 2</sup>

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### INTRODUCTION

Most of our work on the relationship between stress and the nervous system has been concerned with the endocrine aspects of this problem. It is the primary object of this review to present a synopsis of these investigations, but my presentation would be very one-sided if it did not mention at least some of the most important key-references to the purely neurophysiologic and psychiatric aspects of this topic (1-6, 15).

The concept of stress is as old as medical history. More than 24 centuries ago, Hippocrates had already taught his disciples that disease is not only suffering (*pathos*), but also toil (*pónos*), that is, the fight of the body to restore itself toward normalcy. About a century ago, Claude Bernard pointed out that one of the most characteristic features of all living beings is their ability to maintain the constancy of their internal *milieu*, that steady state which Walter Cannon named "homeostasis." It was then felt that perhaps any deviation from the steady state—or, at least, the effort of restoring a homeostatic equilibrium—is stress. If so defined, the concept of stress would include all physiologic deviations from the normal resting state and, yet, in conversational English, it implies a particularly strenuous and usually damaging condition. Furthermore, the term was used by some authors as virtually synonymous with nervous stress and strain, while others employed it to denote the consequences of any noxious agent. Additional confusion was created by the indiscriminate use of the same term for the agent (trauma, emotions, infections, cold) and the effect (morphologic and functional changes in the body) of ex-

posure. This vagueness in the formulation of the subject is probably responsible for the fact that, although the importance of stress in medicine had always been recognized, it had not been possible to submit it to systematic investigation until quite recently.

### AN "OPERATIONAL DEFINITION" OF STRESS

It was the discovery that stress always manifests itself in the form of a definite, stereotyped syndrome that helped us to arrive at what philosophers call an "operational definition" of this condition. It became evident that stress, no matter how produced, elicits certain quite typical and specific changes in the body, such as adrenocortical stimulation, involution of the lymphatic organs, and gastrointestinal disturbances. These alterations served as objectively measurable indicators of stress and led to the following definition: *Stress is the state manifested by a specific syndrome which consists of all the nonspecifically induced changes within a biologic system.* In this sense stress has its own characteristic form but no particular cause.

This is an essentially "operational definition," in that it tells us what must be done to produce and recognize stress. A state can be recognized only by its manifestations; for instance, the state of stress by the manifestations of the stress-syndrome or "general adaptation syndrome" (G.A.S.). Therefore, we must observe a great many living beings exposed to a variety of agents before we can see the shape of stress as such. Those changes which are specifically induced by only one or the other agent must first be rejected; if we then take what is left—that which is nonspecifically induced by many agents—we have unveiled the picture of stress itself.

It was tempting, at first, to define stress merely as "*the rate of wear and tear*" within the body, because this is the immediate non-specific result of both function and damage. Reactions which tend to diminish or repair wear and tear (e.g., corticoid-secretion) are not strictly stress but rather defenses

<sup>1</sup> Read by the guest speaker at the Devereux Dinner during the Annual Meeting of The American Psychiatric Association, Chicago, Ill., Apr. 30-May 4, 1956.

<sup>2</sup> The experiments on which this article is based were supported in part by a grant from the Gustavus and Louise Pfeiffer Research Foundation.

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against stress. In practice it is impossible, however, always to differentiate sharply between a change which represents repair and one which is merely damage. Therefore, this formulation—though more concise and theoretically more satisfying—could not have acted as a basis for a truly operational definition such as was needed to give the concept of stress a solid, objective foundation.

The enormous literature on stress, the general adaptation syndrome, and the so-called "adaptive hormones"—which now comprises more than 25,000 references to original articles and books—has been reviewed in detail elsewhere (9-14). In all these reviews special emphasis has been placed upon the relationship between stress and neuropsychiatric problems. Hence it will not be necessary to complicate this review by a confusingly voluminous bibliography, and I shall limit myself here to a brief discussion of certain experimental observations which, to my mind, may have interesting applications in psychiatry.

#### STEROID ANESTHESIA

In the course of experimental studies on the pharmacology of steroids, it had been noted accidentally that corticoids and related compounds exert singular effects upon the central nervous system in animals. For example, sudden, intense overdosage with *desoxycorticosterone acetate* (DOCA) produced an initial state of excitation, followed by complete surgical anesthesia in the rat, mouse, dog, and in many other mammals. This response appeared to be a fundamental biologic reaction common to all species so far examined, because later it could also be demonstrated in fish, birds, and even in primates, such as the monkey.

These findings raised the question whether excessive endogenous production of certain steroid hormones might play a role in determining mental reactions during stress. When subsequently ACTH and various glucocorticoids (cortisone, cortisol, prednisolone) were introduced into clinical practice, it became evident that these substances may also produce excitement or depression in man.

Additional animal experiments showed that *progesterone* can likewise act as an anesthetic

and this was again confirmed in man (7). It was then thought that perhaps the somnolence of pregnancy and certain nervous derangements in eclampsia, premenstrual tension, and other clinical conditions, accompanied by increased steroid-hormone production, may find their explanation in this phenomenon. Still, in 1941, when these experiments were first performed, we did not even dare to hope that they might form the basis for a new type of anesthesia, which could be employed in clinical surgery, because the extraordinary expense involved in the synthesis of steroid hormones precluded their practical use at the required dose-levels.

However, the great progress made by synthetic chemists has now rendered various steroid derivatives abundantly available at a relatively low cost and one of these, *hydroxydione*, proved capable of producing anesthesia in man (8). Future research will have to show the possible advantages and disadvantages of this compound in comparison with other anesthetics, but, in any event, in the steroids we now have a new tool for the study of nervous responses to natural compounds.

#### THE ANTICONVULSIVE AND TRANQUILIZING EFFECT OF STEROIDS

In the course of our animal experiments on steroid anesthesia, we also noted that the excitement and the convulsions, normally produced by such stimulants as *metrazol* or *picrotoxin*, are effectively combated by various steroids. Independently, D. M. Woodbury and his associates, at the University of Utah, demonstrated similar effects with regard to electroconvulsive seizures.

It may be profitable to explore the possible use of steroids as anticonvulsive agents in epilepsy and perhaps even as tranquilizing drugs in diverse conditions of excitement. Clinical studies along these lines have not yet been conducted, but H. Laborit, in France, has just published rather encouraging observations on the use of hydroxydione in the treatment of delirium tremens.

#### CORTICOIDS AND MUSCULAR PARALYSIS

In dogs given prolonged treatment with DOCA, R. Loeb and his associates, in New

York, have observed a syndrome, reminiscent of the *periodic muscular paralysis*. We have obtained quite similar changes in a primate, the Rhesus monkey, and noted that attacks of paralysis can be produced in DOCA-treated animals at will, by giving them large amounts of sodium chloride. In the production of these paralytic spells there appears to be some synergism between mineralocorticoids and sodium. Conversely, intravenous infusion of a potassium-chloride solution can restore the DOCA-overdosed dog or monkey to normalcy within a few minutes.

It is especially noteworthy, in this connection, that Dr. J. W. Conn observed quite comparable manifestations of muscular paralysis in a woman in whom an adrenocortical tumor produced an excess of aldosterone (a mineralocorticoid, chemically and functionally closely related to desoxycorticosterone). Interestingly, in Dr. Conn's patient (as well as in several others observed since), the *hyperaldosteronism*, which led to these motor disturbances, did not produce edema; conversely, in nephrosis, when urinary aldosterone elimination becomes excessive, there is much edema but no muscular paralysis. Future research will have to show why an excess of such a mineralocorticoid can produce more or less selectively one or the other type of manifestation in different patients. Animal experiments have already shown that hormones, such as DOCA, can act rather specifically on one or the other target, depending upon what we call "conditioning factors." For instance, DOCA produces nephrosclerosis, hypertension, and periarthritis nodosa in rats, or muscular paralysis in dogs and monkeys, much more easily when the sodium-intake is high, but the anesthetic effect of the same hormone is not thus enhanced by the salt-intake.

#### MORPHOLOGIC CHANGES IN THE BRAIN PRODUCED BY CORTICOIDS

In rats heavily overdosed by desoxycorticosterone (especially if they are sensitized by a high NaCl intake and unilateral nephrectomy), there develops an encephalopathy with periarthritis nodosa of the cerebral vessels, marked edema and often multiple massive hemorrhages in the brain. These lesions are

accompanied by convulsions or paralytic changes in the skeletal musculature and by an extreme irritability of the animals. It is possible to prevent such changes by the administration of acidifying salts, for instance, ammonium chloride or calcium chloride.

The question arises whether cerebral changes, such as are seen in clinical periarthritis nodosa and in hypertensive disease, are related to the excessive production of mineralocorticoids, or an excessive conditioning for their actions. In any event, this experimental encephalopathy now serves us as a useful test object for the screening of drugs which may have clinical applications in these diseases of man which are simulated by DOCA-overdosage.

#### STRESS AND THE INFLAMMATORY DISEASES

It is now a generally accepted fact that certain adaptive hormones produced during stress (ACTH, cortisol) have definite anti-inflammatory actions; it is less certain but highly probable that, under special circumstances, stress and the so-called "prophlogistic" hormones (e.g., STH, DOCA, aldosterone) actually stimulate inflammation and the development of the so-called "collagen diseases" in man.

Our attention was called to this relationship between inflammation and the adrenal in the course of experiments on the "anaphylactoid inflammation." It had been noted, in 1937, that the intraperitoneal or intravenous administration of egg-white produces a peculiar hypersensitivity reaction in the rat. This is characterized by a pronounced inflammatory edema in the snout, the paws, and the ears. It was immediately obvious that the adrenal plays an important role in this response, because after adrenalectomy stress failed to prevent this reaction to egg-white. From this we had concluded that stress presumably inhibits inflammation, through the excessive production of ACTH and antiphlogistic corticoids. At the time of our first experiments, purified preparations of ACTH or synthetic anti-inflammatory corticoids were not yet available, but more recently we were able to show that these hormones also inhibit this type of inflammatory hypersensitivity reaction, just as exposure to stress does.

All these facts have been confirmed with a variety of other tests which we have developed for the quantitative assessment of inflammation caused by ordinary chemical irritants (*e.g.*, topical irritation arthritis with formalin, "granuloma pouch" produced with croton oil). In all these instances, inflammatory changes due to tissue irritation have been inhibited, not only by stress due to somatic causes (trauma, burns), but also by such neuromuscular stress as is induced by forced immobilization. Still, stress, no matter how induced, did not exert this inhibitory effect after removal of the adrenals.

The converse effect, namely, the stimulation of inflammation by stress and by adaptive hormones, has also been demonstrated in animal experiments, but it is not yet clear to what extent these findings are applicable to the problems of clinical medicine. Prolonged overdosage with DOCA (especially after sensitization by excess salt-intake) produces periarteritis nodosa and myocarditis; it also sensitizes for the production of various types of experimental arthritis in the rat. Furthermore, under suitable experimental conditions, the antiphlogistic effect of cortisone or cortisol can be inhibited by DOCA or aldosterone in animals. It is clear, therefore, that—at least in certain mammals—the "inflammatory potential" (the ability of tissues to undergo inflammation) depends largely upon the balance of pro- and anti-inflammatory hormones. It is highly probable that, in man, the hormonal regulation of inflammation obeys essentially the same rules, but the clinical effectiveness of the prophylactic principles has not yet been explored as completely as that of the inversely acting hormones.

The importance of the balance between pro- and anti-inflammatory hormones for the regulation of inflammation has several interesting implications in the field of psychosomatic medicine. For example, it had long been known that certain infectious diseases, for instance, tuberculosis, may be greatly aggravated by exposure to virtually any kind of severe stressor. The rest cures for tuberculosis are based upon the empirically established fact that protection from stress is an important aspect in the healing of tuberculous lesions. Animal experiments have shown

that, for instance, in the rat (a species normally resistant to tuberculosis), overdosage with cortisone can induce great sensitivity to tuberculosis bacilli, while the antiphlogistic STH restores resistance to normal, despite continued cortisone-treatment. Even normally saprophytic microorganisms tend to spread and to become highly pathogenic in rats overdosed with ACTH or cortisone and, here again, STH exerts a protective effect. It is probable that, to a large extent at least, the antiphlogistic hormones favor the spreading of infection because they remove the inflammatory barricades around the foci of microorganisms, while the pro-inflammatory hormones act inversely by stimulating granuloma formation and the encapsulation of potentially pathogenic germs. Numerous clinical observations have shown that, in man, heavy and prolonged overdosage with antiphlogistic hormones can also induce the spreading of an originally innocuous and well-delimited tuberculous process.

One of the first observations concerning the alarm reaction—the first stage of the general adaptation syndrome—was that stress produces gastric and duodenal ulcers in animals. It had since been shown—both in experimental animals and in man—that overdosage with antiphlogistic hormones may likewise cause the development and even the perforation of peptic ulcers. This may explain the empirically established relationship between stress (particularly neurogenic stress) and peptic ulcer formation.

In experiments designed to elucidate the mechanism of this phenomenon, we could show that peptic gastric juice, introduced into an experimentally prepared granuloma pouch, does not digest the wall of this sac, because the inflammatory tissue is extraordinarily resistant to peptic digestion. On the other hand, exposure to the stress of forced immobilization causes such a weakening of the granulomatous barricade that it is now readily attacked by peptic juice. In the absence of the adrenals, exposure to similar stress does not thus affect the resistance of granulomatous tissue. It is highly probable, therefore, that the antiphlogistic effect of the adaptive hormones produced during stress plays an important part in the perforation of peptic ulcers; it diminishes the resistance of

the granuloma tissue which normally covers the crater of gastroduodenal ulcers.

The important clinical observations of Dr. Seymour Gray have shown that the secretion of peptic enzymes is enhanced during the alarm reaction by stress and also during treatment with antiphlogistic hormones (*e.g.*, ACTH and cortisone). Consequently, during stress, the perforation of peptic ulcers is presumably facilitated through a dual mechanism: (1) the resistance of the protective granuloma wall is diminished; (2) the secretion of the aggressive enzymes is augmented.

#### STRESS AND SEXUAL DERANGEMENTS

During stress, when the anterior pituitary has to secrete a great excess of ACTH in order to maintain life, the gland apparently cannot simultaneously produce optimal amounts of gonadotrophic hormones. Thus, it was found that prolonged exposure to various physical or emotional stressors results in a cessation of estrus, with ovarian atrophy, and involution of the accessory sex organs in female rats. During lactation, milk secretion ceases. In male rats exposed to chronic stress, there is involution of the testes.

This change in the secretory activity of the anterior pituitary from the normal pattern to one in which ACTH secretion is favored at the expense of other hormones, has been referred to as the "stress-shift in anterior-pituitary activity."

There is ample clinical evidence to show that this "stress-shift" also occurs in man. There is a decrease in libido and fertility in both sexes during stress and this may be accompanied by amenorrhea in women and impotence in men. In such patients, a vicious circle may develop, because continuous worry, fear or pain may lead to sexual disturbances which, in their turn, again cause stress.

We are presently engaged in experiments designed to elucidate the mechanism of the "stress-shift" and, although this work is still incomplete, it may be said already that, in addition to a decreased secretion of sex-hormones, their peripheral activity upon the accessory sex-organs also appears to be impeded by the conditioning effect of stress.

#### SUMMARY

After a brief enumeration of key-references to the literature on stress in psychiatry, the following specific problems are discussed on the basis of personal experiments:

1. An "operational definition" of stress, based on measurable indicators of this state.
2. Steroid anesthesia.
3. The anticonvulsive and tranquilizing effect of steroids.
4. Corticoids and muscular paralysis.
5. Morphologic changes in the brain produced by corticoids.
6. Stress and the inflammatory diseases.
7. Stress and sexual derangements.

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## FEAR AND ANXIETY IN THE COURSE OF ELECTROSHOCK THERAPY<sup>1,2</sup>

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Patients undergoing electroconvulsive therapy (ECT) frequently exhibit marked fear of the treatment. While many medical procedures except the most innocuous ones are accompanied by fear on the part of the patient, the fear of ECT exceeds that of other medical procedures including surgical ones. Therefore, an attempt at a systematic study of this fear seems to be indicated. While the American literature contains numerous reports on psychological tests given after ECT (1-4 and many others), comparatively little attention has been given to fear. Gottesfeld and Baker (5) found fear in 80% of their ECT treated patients; Fisher, Fisher, and Hilkevitch (6) found it in 50% of their patients. Kalinowsky and Hoch (7) point out that fear of ECT "is a greater problem than was originally realized." They stress, as we do in this presentation, the fear "which develops increasingly after a certain number of treatments," and they suggest that this particular fear is connected with the experience of waking up and being unable to become oriented.

<sup>1</sup> Read in abbreviated form at the Twelfth Annual Meeting of the Electroshock Research Association in Chicago, Ill., April 29, 1956.

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### MATERIAL

One hundred consecutively treated private patients, 68 females and 32 males between the ages of 21 and 80, were observed and interviewed before, during, and after the course of therapy in regard to their fear of the treatment. Many patients are rather disinclined to talk spontaneously about their fear, and intense direct questioning was needed in order to obtain sufficient information. Seventy were treated as ambulatory patients in the office, 20 as inpatients in a private sanitarium, and 10 as inpatients on the private floors of the Neurological Institute, Columbia Presbyterian Medical Center. The various diagnostic categories, presence or absence of fear, and treatment results are recorded in Table 1.

The fairly high percentage of manic-depressive depressions in this material is probably explained by the fact that the author had been interested in ECT for 14 years. Therefore, a number of these subjects were patients whose previous episodes had been terminated by ECT.

### FREQUENCY OF FEAR

Of the 100 patients, 67 had marked, but various degrees and various modalities of fear, whereas 33 showed no marked fear. No particular relationship between the sex

TABLE 1

Diagnosis	Number of patients treated	Presence of marked fear	Absence of marked fear	Satisfactory results	Unsatisfactory results
Involutional depression	42	28	14	39	3
Involutional paranoid condition	4	4	0	3	1
Paranoid form of schizophrenia	2	2	0	1	1
Catatonic form of schizophrenia	2	0	2	1	1
Simple form of schizophrenia	5	4	1	2	3
Manic depressive depression	35	22	13	32	3
Schizo-affective psychosis	2	1	1	2	0
Post-partum depression	2	1	1	2	0
Anxiety depression*	3	3	0	2	1
Reactive depression	2	1	1	2	0
Drug addiction†	1	1	0	1	0

\* Under this heading were understood phasic occurrences of a mixed syndrome of depression and anxiety (pseudo-neurotic depressions, conversion depressions of Hauser & Peters) (8). These cases had had intensive unsuccessful psychotherapy in the past.

† Treated with ECT for the technical management of withdrawal (7, 9, 10).

of the patients and proneness to fear of ECT exists.

#### FEAR AND DIAGNOSTIC CATEGORIES

Naturally, the question arises as to the possibility of the relation between the occurrence of fear and the various diagnostic categories. The group of the 9 schizophrenics consisted of 2 paranoid, 2 catatonics, and 5 "simple" cases. Of these, the 2 paranoid showed marked fear, the 2 catatonics showed a complete absence of fear, 4 of the "simple" cases showed fear, and 1 failed to do so.

All 4 patients with involutional paranoid psychoses exhibited fear as did all 3 patients with anxiety depressions as well as the single drug addict.

Of the 2 patients with post-partum depressions, 1 exhibited fear, 1 did not, and the same holds true of the 2 patients with reactive depressions and the 2 patients with schizo-affective disorders.

The completely heterogeneous and statistically insignificant group of 9 schizophrenics, 2 schizo-affective disorders, 4 involutional paranoid psychoses, 3 anxiety depressions, 2 post-partum depressions, 2 reactive depressions, and 1 drug addict shows the occurrence of fear in 15 of 23 patients, which is very close to the over-all occurrence of fear in 67% of all studied (100) patients.

Of the remaining 77 cases of pure depressive pictures (involutional and manic-depressive depressions), 52 exhibited fear and 25 did not. With the exception of anxiety depressions and of cases with a marked paranoid trend, the occurrence of fear in 3 of all treated patients is also equally noticeable if the total group is broken down into smaller diagnostic categories. With the exception of patients with paranoid and anxiety features, the percentage of occurrence of fear appears to be independent of diagnostic categories.

#### FEAR AND RESULTS OF TREATMENT

The high percentage, roughly 90%, of favorable results achieved with ECT in depressive syndromes, and, on the other hand, the rather high percentage of fear, roughly 66%, makes it unlikely that fear can be a

hindrance to effective treatment. The same holds true for the nondepressive groups.

All 3 cases with anxiety depressions had exhibited marked fear; 2 were benefited by the treatment, 1 not. The 2 cases of schizoaffective disorder, including 1 who had exhibited an excessive degree of fear (hiding under the bed or in the room closet prior to the treatment) were restored to normalcy.

Of the 4 patients with involutional paranoid psychoses, who had all shown a high degree of fear, only one failed to recover.

Of the 3 patients with anxiety depressions, who had all shown considerable fear, 1 failed to recover, 1 improved, and 1 made a complete recovery.

Both patients with reactive depressions, the one who had been afraid and the one who had not, made an equally satisfactory recovery.

Both paranoid schizophrenics had shown a high degree of fear; one improved sufficiently to return to work, the other failed to improve and had to be institutionalized. Of the 2 fearless catatonics, one made a complete recovery, the other was only slightly improved. Of the 5 cases of a "simple" form of schizophrenia, the single patient who had failed to show fear did not improve, and of the 4 fearless ones, 2 made a very satisfactory recovery and 2 failed to do so. The same absence of the influence of fear on the therapeutic results could be seen in the remaining group of depressive patients, and we are convinced that fear of the treatment constitutes neither hindrance nor help toward recovery.

#### FEAR AND PREMORBID PERSONALITY

Psychological testing was done in only a minority of cases and failed to reveal any particular clues as to the occurrence or non-occurrence of fear. Impressions concerning the premorbid personality gained on psychiatric examination showed that the patients suffering from anxiety depressions and the one drug addict had always manifested various neurotic fears, hypochondriacal preoccupations, and proneness toward development of anxiety. In addition to these, it appeared that only in patients with paranoid tendencies could a disposition toward fear be traced to their premorbid personality. Of the

remaining patients, those who developed fear of the treatment had by no means shown particular tendencies toward development of fear in their past lives; on the other hand, even some of those who underwent treatment without any fear had shown such proneness in their past lives. In the majority of patients, fear of the treatment had as little to do with their premorbid personality as with the character of their psychosis or with the results of therapy.

#### REASONABLE CONCERN AND APPREHENSION— NON-PATHOLOGICAL FEAR

The term "electric shock" as such, is likely to provoke concern, apprehension, and fear, though not of pathological nature, in the patient and his relatives. Either component of the term, one referring to electricity and the other to a state of shock, is equally likely to induce fear. The fact that an electric current is applied to the head is also mentioned by patients as cause for particular concern, and many patients are inclined to produce associations comparing this form of therapy to electrocution. Some form or degree of reasonable concern can be expected to be voiced by every patient except by advanced depressives and catatonics. Patients with a feeling of reasonable apprehension, if it could be dispelled easily by information and reassurance, were not therefore included among those(67) listed as exhibiting fear or anxiety. Such apprehension is not different from the concern associated with any other form of vigorous therapy.

Rarely will patients voice any particular concern about the fact that they will have to undergo convulsions. This fear is expressed only by those who have witnessed epileptic seizures in members of their family or have observed the administration of ECT.

Not only well-meaning though ill-advised neighbors, but occasionally also psychologists or psychiatrists, averse to electroshock therapy for theoretical reasons, may bolster this reasonable apprehension and fear. The negative attitude of this group of psychologists and psychiatrists toward a form of therapy which within a few days eliminates suicidal risk, which has made tube feeding unnecessary, and which in the vast majority of cases terminates depressive episodes within 3 or

4 weeks is hard to understand. Even in 1955, after the introduction of Anectine, which allows the administration of electroshock with not more than mild twitchings of toes and fingers instead of a generalized convulsion, Wayne(11) states that the treatment "has all the characteristics of an overwhelming assault," also "an appearance suggestive of the sexual act" and "a ritualistic and magical quality." The same author quotes the opinion of H. S. Sullivan(12) that electroshock therapy does no more than create happy morons or contented imbeciles. Moss, Thigpen and Robison(13) have quoted a whole series of authors who "deplore electric treatment and glibly assume that such treatment is given to satisfy unconscious sadistic cruelty in the physicians who use it." These latter authors state further:

The same theories could, if one wished to philosophize, be applied most interestingly to the question why these commentators should be so bitterly opposed to a measure that relieves permanent disability and inexpressible anguish in so many patients.

While opinions and "theories" of this sort certainly do not deserve to be discussed under the topic of reasonable concern, they may very well when conveyed by professional people to patients or their relatives cause reasonable concern and fear in the mind of those patients who have no means of ascertaining the professional competency of their sources of information.

#### SOMATOGENIC FEAR

The occasional muscle aches, headaches, and nausea which may disturb the patient after the application of the standard treatment without the use of intravenous anesthesia, or the hang-over effect of an intravenous anesthesia, can be the cause of a mild, usually easily manageable fear. The organic memory impairment may disturb patients, particularly those who perform intellectual work, and they may attach to this complication fear that the memory disturbance may be of permanent character. Also, the occasional fleeting dysgnostic experiences encountered as an after effect of electroshock (9) may cause fear and concern. Every patient is always ready to admit that no dis-

comfort or anything at all is experienced during the treatment proper.

#### UNREASONABLE CONCERN AND FEARS IN PRE-MORBIDLY ANXIOUS PATIENTS

Manifestations and conscious motivation is qualitatively not different from what has been described as reasonable concern. There is, however, a marked quantitative difference. It can not be dispelled or corrected. The fear is profounder than in what has been referred to as reasonable concern. These patients will continue during the course of treatment to insist on the similarity of the treatment to electrocution. They will state and restate their fear of the word "shock," of the application of electricity to the head. They will object to seeing the shock machine, and if they are treated without intravenous anesthesia, voice their horror of the application of the electrodes and jelly. Prior to each treatment, they will ask whether this is going to hurt and will not be appreciably reassured by reference to previous painless treatments. This type of fear is seen in patients who have been premorbidly anxious through their entire lives, and who have been afraid of any kind of medical procedure. As can be expected, the patients suffering from anxiety depressions exhibit this fear. When ECT was administered in intravenous anesthesia without the patients' knowing that they had received any form of treatment other than an intravenous injection, they underwent treatment willingly and without fear. In these cases it is obvious that fear of the concept of electric shock or of seeing the necessary preparations and paraphanalia was the essential factor. Another type of fear seen in this premorbidly anxious group is the "fear of going to sleep," even a "fear of talking in my sleep," a "fear of losing control," even a "fear of lying down." These patients will make such statements as follow: "It frightens me to go to sleep, the whole thing, the thought of it." "Is this going to hurt?" "I do not want to go to sleep." Some patients seem to feel that the suddenness of the loss of consciousness, as obtained with the standard form of treatment without intravenous anesthesia, is particularly frightening. "A terrible treatment, no pain, I lose consciousness

immediately." These patients, when investigated as to their attitude toward general anesthesia, revealed that in the past they had had a similar attitude toward general anesthesia. They may have a persistent neurotic fear of death and are afraid that any type of procedure, particularly one which when combined with loss of consciousness, may result in death. This sort of fear does not increase with the progress of treatment, but rather is likely to decrease.

#### FEAR OF THE TREATMENT INTEGRATED INTO THE STRUCTURE OF THE PSYCHOSIS

In paranoid patients, and occasionally in depressive patients, the fear of the therapy becomes integrated into the structure of the psychosis. Paranoids, schizophrenics, and patients with involutional paranoid conditions tend to consider the treatment as one of the many hostile, persecutory acts directed against them. One of our patients after recovery mentioned that she had been firmly convinced that the treatment had been administered as a form of electrocution by the F.B.I. She also showed features of fear mentioned as characteristic of the premorbidly anxious group, to which she also belonged. She stated that she "hated the electrodes," that she "hated the odor of the jelly." She accepted treatment with much less fear and hesitancy when it was administered in intravenous sodium amyta sleep. Occasionally, depressive patients with a strong sense of guilt will consider the treatment as a form of deserved punishment. These patients usually show only a small degree of fear in the beginning of the treatment course when they are still severely depressed. With the progressive improvement of the affective situation they become less inclined to accept treatment. One patient who had accepted the treatment at the onset without difficulties, during the second half of the course spoke of "torture" and of the treatment room as the "torture chamber."

#### THE FRIGHTENING EXPERIENCE OF UNFAMILIARITY—DISCONNECTION FROM THE PAST AND FUTURE

All types of fear so far enumerated apply only to the minority of the patients who are

afraid. The majority invariably mention as the frightening factor the loss of memory on waking up. "I hate to have lost my memory when I wake up. When I wake up I do not know where I am and that worries me. Falling asleep and not recognizing anybody, I get frightened." "When you wake up it is the most terrible feeling like if you were an idiot." In this important group of patients no pathological fear prior to treatment and during the early treatment period is noticeable. Their fear develops increasingly with the progress of treatment.

The patient who wakes up after electroshock therapy, particularly when the standard method without intravenous anesthesia has been used, differs fundamentally from the one who wakes up after general surgical anesthesia. The latter will very quickly recover memory and orientation shortly after having regained consciousness. If he is disoriented, it will be sufficient to tell him where he is and what has happened in order to establish his orientation. The patient who wakes up after shock therapy, on the other hand, finds himself conscious, but for a fairly long time, sometimes up to an hour, he may be bereft of his memory and his orientation may be severely impaired. He will look around amazed, with a frightened expression. "Am I all right now? What happened to me? Where did I pass out?" He will not accept the true information when it is given to him and will constantly repeat the same questions without being able to comprehend the answers. The patient waking up after surgical anesthesia, particularly on being told that he is now waking up from his operation, can almost immediately re-establish his connection with the past. He remembers the illness and the fact that he had to undergo surgery and will quickly orient himself in the recovery room. The patient waking up after electroshock will not remember that he had a depression. He will not remember that shock treatment had been decided upon and that he has come to the hospital or to the office for the purpose of treatment. He will not remember (when treated at the office) that one member of his family has accompanied him and will constantly ask: "How did I get here? What has happened? Where am I? Where is my wife?" He will not ac-

cept answers, since they fail to ring a bell in his mind. He continues to remain disconnected from his own past. And also differing from the post-surgical patient, he will be unable to project himself into the future. He will not accept the suggestion that he will be allowed to go home soon, that he will have a good night's sleep, but instead will remain in a state of utter bewilderment, disconnected equally from the past and the future, finding himself in an unrecognized, strange environment. His behavior will be identical on each treatment. One of our patients with a severe reactive depression, due to the death of her husband, had to be told each time after each of her 10 treatments of her husband's death. Each time anew she went through an agony of grief, as if suddenly confronted with an unexpected shattering new fact.

As mentioned, the seemingly inexplicable fear and anxiety are nonexistent with the first and second treatments but increase gradually with the progress of treatment. The disconnection from the past and the temporary inability to recover retrograde recent mnemonic material and even remote material becomes more pronounced as treatment proceeds. After the first treatment or the first few treatments, no particular fear appears to be connected with the retrograde amnesia. The repetitive administration of electroshock deepens the disconnection from the past and its affective repercussions. It is then that the specific fear of the treatment may assume the aspect of a vague, all-pervading, almost objectless anxiety. This fear not only increases with the progress of treatment, but may also linger on after treatment, occasionally to such an extent that the patient, even when completely recovered, likes to avoid the street in which the hospital or the doctor's office is located. When suffering another depressive episode, this fear, acquired during the preceding episode, may make it difficult for the patient to accept treatment again.

The patient waking up from electroshock differs from the patient waking up from surgical anesthesia not only in his profound temporary disconnection from the past and his inability to project himself into the future, but also, in conjunction with this disturbance, by a profound change of his sense of familiarity. The patient recovering from

surgical anesthesia will not experience his surroundings, the nurses and doctors and recovery room, as particularly strange, unfamiliar, and frightening, whereas the one recovering from electroshock may do so. The setting in which he finds himself impresses him as unique, alarmingly strange and unfamiliar.

Elements can be recognized but the feeling of familiarity is defective. Such patients are not sure how the facts and elements of memory experience and of reality fit together[14].

One of our patients stated: "It is like being in another world. It is only after an hour that things and faces look familiar again." This is indeed a fear-inducing experience. The experience of waking up disconnected from the past, in a strange environment, accompanied by a sense of strange unfamiliarity, was expressed by one of our patients when she spoke of "the waking up from nowhere into the anywhere." In this connection it may be pointed out

... that past and future are not absolutely separated regions touching at a highly remarkable zero point, the name of which is the present. The past is that which was as it appears to me today, the future is that which comes as it comes to meet me now[15].

The disconnection from the past and the inability to project into the future, therefore, are only two different facets of the same disturbance. When recovery of the past begins, the past appears to be more remote than it actually is. In a self-observation, Bersot[16] spoke of "*la sensation d'éloignement du passé*." Even more than the disconnection from the past, the loss of the experience of familiarity appears to be the fear and anxiety-inducing element. The experience of strangeness as described by the patients is profound and disturbing, creating not only specific fear of the treatment but also diffuse and vague anxiety.

Attention has been called to the striking similarity between the expressions used by patients who experience fear and anxiety due to a sense of unfamiliarity and disconnection from the past and the terminology and concepts of existentialistic philosophy[17, 18, 19]. For Jaspers[20] as well as for Heidegger[21], anxiety is a basic quality and fundamental characteristic (*Grundbe-*

*findlichkeit*) of human existence. This viewpoint, of course, differs sharply and irreconcilably from the one prevailing in our so-called dynamic psychiatry, in which anxiety in all its modifications is considered to be a psychopathological manifestation. German psychiatrists compare, at least implicitly, the situation of the patient waking up from electroshock and the manifestations of his anxiety to the situation of man as described in existentialistic philosophy. There, man finds himself thrown into a world which is not of his choosing, surrounded by facts not of his making, and, therefore, he is afflicted with fear and anxiety. Long before existentialism, Pascal[22] wrote as follows:

When I consider the short duration of my life, swallowed up in the eternity before and after, the little space which I fill, and even can see, engulfed in the infinite immensity of space of which I am ignorant, and which knows me not, I am frightened, and am astonished being here rather than there, why now rather than then.

The hypothesis may be offered that the basic anxiety characterizing man's being in the world is normally neutralized by his sense of familiarity. The familiarity of environment of life in general, of ourselves, is reassuring. Under normal conditions we know our place in time. Our recollection of and relation to the past is as clear as our ability to project ourselves into the future. All these familiar aspects may be extremely obscured during the awakening after ECT. A temporary annihilation of the sense of familiarity after electroshock provokes basic anxiety, and results in a strong, progressively increasing fear of the treatment.

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#### DISCUSSION

DR. L. B. KALINOWSKY (New York City).—Dr. Gallinek's presentation is probably the first systematic study of fear and anxiety in connection with ECT. Fear in metrazol convulsive treatment had been related to the sensations the patient experiences with the injection, and, therefore, no attention was paid to the other components of the fear such patients retained from the treatment. Most remarks in the metrazol literature are centered on the fear as the therapeutically effective agent. The therapeutically ineffective tortures of previous times, and the ineffectiveness of painful electric stimuli used in psychotics prior to the shock era proved that fear could not be the effective agent, and when

electroshock therapy was introduced, this theory was abandoned. But as time went on, it became obvious that even patients who have no fear of electroshock in the beginning, or at least after the first treatment when they realize that it is not painful, do develop fear usually after the fourth or fifth treatment when their psychotic fear usually has subsided. Dr. Gallinek's study has shown that this fear is not explained by such factors as premorbid personality, type of illness, or therapeutic results. I can only agree with all the observations made by Dr. Gallinek perhaps with the one exception that I was always surprised to find how little paranoid patients integrate the fear of the therapy into the structure of their psychosis. It is amazing how easily schizophrenics who object to everything suggested to them, often submit with an almost "automatic obedience" to the treatment, and there is often less difficulty to continue treatment with them than with depressed patients.

The treatment fear of any patient, psychotic and nonpsychotic alike, is connected with the confusion on waking up. Usually patients do not offer this explanation spontaneously. Dr. Gallinek interviewed patients about their fear, and he was able to confirm its origin in the posttreatment phenomena which I have always been inclined to accuse *per exclusionem*. Patients waking up from a narcosis, and also patients waking up from insulin coma do not have the frightening experience of the electroshock patient, and it can be assumed that the confusion, so typical for an electroshock convulsion, is the causative factor for the deviation from other types of awakening. In my experience the gradual development of the electroshock fear is not prevented by premedication with intravenous barbiturates. It is interesting, however, that according to Lieser, who uses chlorpromazine immediately after the convolution, fear can be successfully allayed in this way.

Finally I should like to stress Dr. Gallinek's references to an existentialistic interpretation of the electroshock fear and anxiety. Instances of clinical observation which really help us to understand basic mechanisms are disappointingly rare. Anxiety due to electroshock is a phenomenon which gives us a good opportunity to think. I never felt that the existentialistic approach to psychiatry in Europe was any more helpful to true understanding than the psychoanalytical approach, but the phenomena discussed in this paper offer themselves more easily to an existentialistic approach than to psychoanalytical interpretation. Therefore, Dr. Gallinek's study is equally interesting for its practical aspects as for basic theoretical considerations.

## EXPERIMENTAL ASPECTS OF ANXIETY<sup>1</sup>

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This paper proposes to review briefly certain aspects of a variety of experimental studies which have some bearing on the phenomena generally referred to as anxiety. In part, the experiments were designed to investigate the problem of anxiety, but the majority are concerned with determining the behavior, under a variety of conditions, of phenomena which are more or less precisely operationally defined. Most of these experiments are on animals, in spite of the fact that the late Dr. Harry Stack Sullivan as well as more recent authors have maintained that the phenomena constituting "anxiety" are limited to human beings, *i.e.* to acculturated *homo sapiens*. There is no question that the complexity, modifiability, and variability of human behavior in contrast to that of lower forms is of a different order of magnitude and that consequently normal anticipatory behavior—commonly experienced as "the mind"—is dependent on relatively frequent association with other humans. However, some of the grosser patterns of change in mode of interactive behavior in the course of transactions are sufficiently similar by available criteria to justify the use of simplified techniques for their further elucidation.

### ADRENAL HORMONES AND "ANXIETY"

Since the work of Walter B. Cannon and of Hans Selye, considerable attention has centered on the hypothalamic-sympathetic-adrenal medullary system and the hypothalamic-hypophyseal-adrenal cortical system, both in terms of their response in physical stress and in anticipatory activity following a signal associated with probable future stress. The more recent separation of 2 hormones from the adrenal medulla, epinephrine and norepinephrine, has further clarified the field in calling attention to different patterns of cardiovascular responses

associated with different verbal and gestural patterns of behavior. The well-known experiments of Funkenstein (1) and his associates at Harvard demonstrated the different responses of different subjects to a frustrating experience. The subjects who expressed anger at the situation ("anger out" subjects) showed cardiovascular responses similar to those evoked by norepinephrine, whereas the subjects who blamed themselves for poor performance ("anger in" subjects) and those who showed "anxiety" responded in patterns similar to those evoked by epinephrine. Reiser (2) similarly found differences in the cardiovascular responses of enlisted men to experimental interviews. When the interviewer was an enlisted technician the subjects expressed resentment freely and showed the norepinephrine type of cardiovascular pattern. With an officer interviewer the verbal and gestural behavior was restrained and the epinephrine cardiovascular pattern appeared.

These observations raise the question as to the economy of 2 systems, both associated with anticipatory behavior implying physical activity and therefore improved metabolic support for muscles and brain. If we assume that functional systems have developed (phylogenetically) in response to interactions of organisms with the environment—which includes other organisms—we may consider the different requirements of such interaction. It appears that 2 separate problems need to be solved in emergency situations. The first is the assessment of the situation and shift from the previous, on-going activity to *one or another* pattern of interaction appropriate to the new information. The problem of shift of attitude or set is difficult, since the properties of the central nervous system are such as to maintain on-going behavior (*cf.* Sherrington's "competition for the final-common-path") probably necessary to provide some measure of temporal stability. A quick acting system, not only improving cardiovascular support for the brain but also facilitating central scanning mecha-

<sup>1</sup> Read in the Section on Psychotherapy at the 112th annual meeting of The American Psychiatric Association, Chicago, Ill., April 30-May 4, 1956.

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nisms and temporarily inhibiting innate patterns of interaction until one or another is reinforced, would be called for. The second problem in an emergency system is the longer continued metabolic support once one or another of the interaction patterns is established. It is tempting to speculate that the central and peripheral mechanisms associated with epinephrine and with norepinephrine secretion are related to these 2 problems which arise in emergency situations.

It should be emphasized here that the subjective phenomena of "anxiety" are not due simply to the peripheral response to sympathetic-epinephrine stimulation. Early observations on the effects of intravenous adrenalin showed quite typical responses of "anxiety," with tremor, nervousness, agitation, and so forth. Recent studies by Basowitz *et al.*(3) have demonstrated quite different effects, depending on personality factors and former experiences the subjects described with "anxiety" manifestations. Thaler and Goldstein have had occasion to study the responses of subjects to the intravenous infusion of epinephrine in the course of other experiments with similar results. Subjects may merely describe current sensations, such as feeling the heart beat, cold feet, etc., or they may verbally express and act out a full-fledged "anxiety" pattern. The different responses are apparently correlated with previous transactional patterns. It is of interest to note the change in emphasis in the more recent studies as compared with the earlier. This may be due to more attention to detail with repetition of the technique. However, it also calls to mind a change which has occurred more generally in social communication. In World War I neurocirculatory asthenia was an important medical problem, treated largely by internists and cardiologists. In World War II it was rarely seen, the cases of anxiety reaction being handled by the division psychiatrists and for the most part returned to duty. That the "anxiety" type of action communication is no longer a socially preferred mode is also evidenced by its decreased frequency in psychiatric practice. Currently the so-called anxiety neurotic shows a more restrained pattern.

The finding of the increased output of ad-

renal cortical steroids under conditions of physical stress has led to investigations on the relation of these steroids to anticipatory behavior in response to threat and danger. Howard, while studying surgical shock in a field physiological laboratory in Korea(4), collected urine samples from men under a variety of threatening conditions. He found no changes in the 17-ketosteroid fraction in response to increased danger, but in a number of men, though not in all, there were marked rises in the formaldehydogenic fraction. Very high levels, for example, were found in 3 men following an episode in which they were blown out of a bunker by a mortar shell, although they suffered no physical injury. Elmadjian(5) found increased 17-ketosteroids in the urine of men engaged for 16 hours in an attack against Chinese positions in which the attacking company suffered a relatively high casualty rate.

Grinker has conducted probably the most extensive experiments on the correlation of "anxiety states" and adrenal steroid output. Thaler, Price, and Mason, in our laboratories(6), have made some observations which are pertinent. A battery of psychological tests and psychiatric interviews were used, and the results correlated with the 17-hydroxycorticosteroid level in the blood of patients in the surgical wards on the day before elective thoracic surgery. The majority of these patients had been in the hospital for 1 to 3 weeks, which permitted control observations. Quite routinely, high steroid values were found on the day of admission, subsiding to normal levels in 2 or 3 days. (This rise in blood—and urine—17-hydroxycorticosteroids has been observed in normal subjects on the day before the start of a 3-day, sleep deprivation experiment, and also in monkeys on the first day or two after they are brought to the laboratory from the animal farm.) On the day before operation, a quite good correlation was found between indices of "free anxiety" in the psychological and psychiatric examinations and rise of the 17-hydroxycorticosteroids in the blood, although it was only in those patients showing the highest degrees of "anxiety," including a tendency to react with overt physical activity, that the blood steroids were invariably elevated.

In contrast with the rise in adrenal steroids found in subjects responding with signs of increased alertness and activity to unusual, unexpected, or otherwise threatening situations, study of the blood and urine 17-hydroxycorticosteroids in subjects remaining awake for 72 to 90 hours—and thereby suffering a certain amount of discomfort—showed little change or a tendency to fall in the steroid level. During the sleep deprivation period appropriate psychological tests uniformly showed decreased capacity for maintenance of alert attention.

It may also be noted here that in studies on the cardiovascular responses of selected patients from the medical wards of the Walter Reed Army Hospital, Thaler, Reiser, and Weiner(7) found that certain patients with duodenal ulcer in a depressed, withdrawn state of mind showed the least physiological reactivity. In contrast, most ulcer patients showed prompt and fairly strong responses to interaction with the investigator.

In human experiments on anxiety states, the tendency has been to select those situations in which the subjects are faced with threatening, unstructured situations. It needs to be emphasized, therefore, that very pronounced symptoms of anxiety may be quite consistently evoked by situations of isolation, particularly in those cases where the duration of isolation is not defined. In this respect experimental observations confirm the field observations of men like Marshall(8) who found that the terror of the battlefield was its "lonesomeness."

#### ANIMAL EXPERIMENTS: EXPERIMENTAL NEUROSES

I do not propose to review the extensive literature upon so-called experimental neurosis, but rather to refer to a few studies which illustrate certain principles.

One of the problems in reliably evoking disturbed ("emotional," "anxiety") behavior has been to determine the nature of the situation in which the painful, threatening, or dangerous signal will be effective. It would appear that the most favorable situation is one in which the animal is strongly involved\*

in arriving at a relatively soon anticipated goal. Thus Wikler(9) produced very strong aversive and "fear" reactions to female dogs in heat in a young male dog by giving him a moderately painful electrical shock to the hind legs at the moment he achieved intromission for the first time. Single-experience aversive conditioning was routinely obtained in rats by Hudson(10) by giving them a painful shock through a metal feeding tube from which they had become accustomed to eat. Indeed, Hudson found that visual objects introduced within 1 or 2 seconds *after* the painful shock also became signals for aversive behavior. Some of the most important experiments on aversive conditioning have been described by Masserman and Pechtel(11, 12, 13). The importance of their work lies in the fact that the threatening stimulus when applied on other occasions evokes little to no response in the animal. Thus, a puff of air in the face of a cat under ordinary circumstances results only in shaking the head and licking the nose. If the same stimulus, however, is delivered when the cat is about to take a morsel of food in a situation in which it has learned to open the food box on the presentation of a visual stimulus, very dramatic results ensue. The cat springs away from the box with signs of marked disturbance, and if the experience is repeated once or twice, develops strong aversive reactions to the food box and adopts rather bizarre patterns of behavior. Equivalent but more complex manifestations are evoked in monkeys on the presentation of a threatening visual stimulus (a small green rubber snake). The importance of the timing of the aversive stimulus in the course of the interaction between the animal and the apparatus seems to be crucial. The same principle of accurate tim-

tive in portraying the interaction of patients with the examiner during studies on cardiovascular responses to a structured interview. She found that withdrawn (schizoid or depressed) patients showed little or no cardiovascular reaction but that patients who became "involved" with the examiner showed pronounced changes. In the animal experiments the term "involved" is used to indicate the degree to which all other activity than that directed toward a shortly anticipated goal is reduced, as well as the degree to which anticipatory preparations for extraneous interruption is reduced.

\* I have adopted the term "involved" from Dr. Margaret Thaler, who found it descriptively effec-

ing applies in the free-operant conditioning technique utilizing what has come to be known popularly as the "Skinner Box." The effectiveness of the automatic methods used apparently depends to a large extent on the possibility of delivering the painful stimulus synchronously with, or at a fixed short interval after, the conditioning signal.

From a somewhat different point of view certain experiments of Liddell(14) demonstrate the importance of temporal factors. A young ram trained to make discriminative responses to metronome clicks at different frequencies performed accurately on the routine schedule of tests at 5-minute intervals. When, however, the interval was reduced unexpectedly to 1 minute the ram showed marked disturbance and discriminative behavior was completely occluded.

In addition to the factors noted, reference should be made here to the classical experiments in which markedly disturbed behavior has been evoked. In these experiments the disturbed behavior followed presentation of signals increasingly similar but of different anticipatory significance, such as reward *vs.* no reward. The breakdown in discriminative behavior is frequently accompanied by vigorous activity as well as by quite bizarre stereotyped patterns appearing in situations other than the testing situation. Gantt(15) has conducted a detailed study extending over several years of these phenomena, and his data on his famous dog Nick will survive volumes of theoretical speculations on "anxiety."

In most animal experiments resulting in disturbed behavior the initial response to the change in the environment which precedes the disturbance is usually one of strong striped muscle and autonomic (chiefly sympathetic in overt manifestations) activity. Thus, the animal usually withdraws rapidly, jumps, runs around the apparatus, struggles if restrained, and so forth. This is accompanied by increased pulse rate, moderate piloerection, dilated pupils, etc., although defecation and micturition may also occur in some species or situations. The later behavior of these experimental preparations varies widely, both between species and between experimental situations. In general it may be noted that the variety of interac-

tion patterns in which the animal's behavior may be reorganized after the initial common "alarm" or "emergency-action" response is greater in the animals higher in the phyletic scale in any standardized situation. Thus the great majority of individual rats respond in the same stereotyped pattern when subjected to the same experimental procedure. Cats, dogs, monkeys, and chimpanzees have an increasingly varied repertoire. The human has a still larger variety of "defenses" or "anxiety binding" patterns, but is somewhat limited in utilizing these capacities because of group pressures to conform to accepted modes of communication. This has been noted particularly in combat psychiatry. Men joining a unit and shortly thereafter becoming psychiatric casualties show the syndromes typical of the unit, together with the appropriate autonomic manifestations.

One other aspect of the change in mode of behavior—which is part of the "anxiety" phenomena—needs to be mentioned. This is that the new patterns of interaction are only fortuitously effective, depending on the environmental response. In laboratory experiments both the acute responses and the chronic, stereotyped patterns of behavior have been found frequently to be ineffective and occasionally lethal. Thus Richter(16) found that wild Norway rats can be literally "scared to death"—the heart precipitously slowing and stopping—in a threatening, but not physically traumatic situation. Richter (17), Masserman (11, 12, 13), and others have also shown that chronic states of refusal to eat, accompanied or not by other bizarre behavior such as maintained postures, etc., can be evoked by previous threatening experience associated with food or the feeding situation.

#### ANIMAL EXPERIMENTS: EXPERIMENTAL MODIFICATION OF CONDITIONED RESPONSES OF THE "ANXIETY" OR "EMOTIONAL" TYPE

In this section I shall review, albeit much too briefly, a series of experiments initiated by Brady and Hunt several years ago and still continuing under Brady in our laboratories(18). The free-operant conditioning method developed by Skinner has been used and found most elegant for this type of in-

vestigation. Most of the work has been conducted on rats but similar effects have been obtained in cats and monkeys.

In the first experiments, two conditioned responses were developed in the rats. One response was pressing a lever to obtain the reinforcement—either a drop of water or a pellet of food. The other response was an aversive reaction. In Group A, the rats were exposed to a signal (usually a clicker noise) for 3 or 5 minutes, at the end of which time, coincidentally with the last click, the rat received a moderately painful shock through the grid-floor of the cage. Within 6 to 8 trials of the stimulus and painful shock, the A rats would sit crouched, trembling, fur ruffled, and frequently defecating. It should be emphasized that no such response to the clicker was observed before it was paired with the nociceptive stimulus. The B group of rats were also exposed to a similar stimulus, *i.e.*, a clicker, but these animals received the painful shock to their feet only if they pressed the lever for the reward. Again, within 6 to 8 trials, the B rats stopped pressing the lever, but instead of crouching frozen and trembling, they ran about the cage, fur slick, no defecation, and occasionally approached the lever, touched it, but withdrew without pressing it. Following the establishment of these 2 types of aversive response, the A and the B groups were subjected to a series of electroconvulsive shocks and then retested. When exposed to the clicker, the A rates had completely lost their aversive reaction and continued to press the lever in a normal fashion. The aversive behavior of the B rats, however, was unmodified by the electroconvulsive experience. Attention may also be called to the fact that in neither group was the conditioned manipulation of the lever for a reward effected. In more recent experiments Brady has found that reserpine also clearly differentiates these 2 types of conditioned responses in rats and monkeys, the conditioned emotional response disappearing under reserpine, the other being maintained. In some recent experiments on monkeys in collaboration with Mason, it has been found that during a period of several days in which the CER is repeatedly evoked, the blood steroids rise, but under reserpine,

coincidentally with the diminution and loss of the CER, the blood steroids fall.

A number of other phenomena have been found with regard to the behavior of this CER in rats over the course of time—the abolition of the CER by electroconvulsive shock has been found to be temporary, the response returning in about a month following the last convolution. However, of possible importance to therapeutic strategy, it has been found that the CER can be extinguished during that period when it does not appear. That is, if the rat during the month following the electroconvulsive shocks is exposed to the clicker but the clicker is not followed by a painful stimulus, the CER does not reappear as it does in the rats which have not been extinguished. Another important observation is as follows: The CER can be repeatedly developed and extinguished by appropriate pairing of the clicker with the painful shock or giving the clicker alone. When the production and the extinction of the CER are repeated sequentially, the rate of "learning" and "unlearning" is markedly increased, so that only 2 or 3 reinforcement or extinction experiences are necessary in the sequence. In striking contrast to this result is the effect of rare but random reinforcement. Under such conditions the CER becomes remarkably stable and difficult to extinguish, although the animal has received many fewer reinforcements than he did in the series of regular sequential reinforcement-extinction experiments. These observations strongly emphasize the importance of the temporal structure of the experimental situation. Another important effect of time has recently been discovered. In the early experiments the CER was evoked once or twice a day in the experimental animals and was found to be remarkably stable for periods of months. Recently the effect of evoking the CER repeatedly during 2 to 3-hour sessions every day has shown that in a relatively short time the animal's behavior changes. Instead of remaining frozen throughout the period of the clicker, he comes to move about and to continue to press the lever for the reward during the first half of the 3- or 5-minute period during which the clicker sounds. He then returns to the frozen "anxiety" posture during the second

half of the clicker and until the painful shock is delivered. Several series of experiments in our laboratories (19, 20) have been designed to further investigate the phenomenon originally described by Olds (21): that when electrodes are steriley implanted in certain parts of the limbic system of the rat's brain and connected to a stimulator which the rat may activate by pressing a lever, the stimulus behaves as a reward and the rat will stimulate itself for long periods. The most effective points for stimulation appear to be in regions close to the medial forebrain bundle, apparently more medially in the rat and more laterally in the cat and monkey. It has been found that animals will work on experimental schedules in every way similar to the schedules for water, food, or avoidance of painful stimuli. In a number of experiments the self-stimulation reward has been combined with other conditioned responses. The results, in combination with the CER, have been most dramatic. Rats with appropriately implanted electrodes have "learned" to press a lever, either for a water reward or for self-stimulation. They have also "learned" the emotional response to a clicker. When such preparations are working for water and the clicker is presented, they immediately freeze in the typical manner. However, in a number of cases where the animal is working for the self-stimulating reward, the presentation of the clicker in no way interrupts the on-going behavior. When at the end of the clicker the painful stimulus comes, the rat gives a typical jump and then continues with his self-stimulation. It might be argued that the self-stimulation "makes the rats unaware of the situation." However, it has been found that when a rat previously deprived of water is presented with 2 levers, one of which gives self-stimulation, the other of which provides water, he initially works on the self-stimulation lever but after a time comes to operate both, indicating a quite adequate "awareness" of the situation.

#### ANIMAL EXPERIMENTS: USE OF IMPLANTED SUBCORTICAL ELECTRODES FOR RECORDING C.N.S. ACTIVITY

Technical improvements in the construction and use of deep implanted electrodes

have recently provided students of animal behavior with a very powerful tool for obtaining continuous records of activity in various loci in the brains of animals during particular patterns of behavior. These methods have been applied by Galambos and Sheatz (22) to the study of the behavior of cats before and after aversive conditioning to auditory signals—namely, a series of clicks. Electrodes were implanted in a number of discrete anatomical structures in the brain, including the auditory cortex, the medial geniculate, the hippocampus and the cochlear nucleus. The animals were exposed to clicks at a rate of one a second for long periods in their home cages. When brought to the experimental cage and continuous recording started, it was found that electrical activity was evoked in the cortex, the geniculate, and the cochlear nucleus quite irregularly, being fairly strong at times and again disappearing in response to the regular clicks of a series. The animal was awake, lay relaxed, or stood, but gave no overt response to the auditory signal. Following these control observations the animal was given a mild electric shock across the chest coincidentally with every sixth to tenth click. After a few such experiences strong activity was evoked not only in the structures of the auditory system, but also in the hippocampus and certain other areas investigated. The overt behavior also changed to one of standing or crouching, the muscles tense and the posture rigid. Discontinuing the unpleasant stimulus resulted in the anticipatory aversive response being extinguished and the electrical activity pattern returning to its previous form. These observations demonstrate the widespread reorganization of central nervous activity during anticipatory aversive behavior. In particular they emphasize central control of activity at the level of the first synapse of the input system. Indeed, the demonstration of the inhibitory functions of the olivo-cochlear tract on transmission of messages from the organ of Corti to the acoustic nerve (Galambos, 1955) shows that the central control of input starts at the peripheral receptor organ.

That the activity at the level of the first central synapse can be selectively modified to particular tones and also depressed by other patterns of alertness or attention has

been demonstrated recently by Hernández-Péón, Scherrer, and Jouvet(23) in Dr. H. W. Magoun's department at the University of California at Los Angeles. The importance of these data is the implication that the input is controlled to brainstem nerve-nets (usually classified as reflex or innate mechanisms) as well as to the forebrain (often referred to as "consciousness" mechanisms) by particular patterns of reciprocal activity between these systems.

#### CONCLUSIONS

Consideration of experimental studies on animals and humans indicates that a number of different phenomena are ordinarily included in the general concept of "anxiety." The varieties of precipitating events and the varieties of responses are sufficiently great that these phenomena are not useful as criteria of a single, general class of events. There is one phenomenon, however, which does differentiate a class of events that would be useful to identify whether or not it is called "anxiety." This phenomenon is change in the *form* of behavior (*i.e.* the *mode* of the interaction pattern) which occurs in the course of a transaction. Such a change is always present in situations with humans in which "anxiety" is diagnosed, but may also occur in situations in which the verbal, gestural, and autonomic manifestations are not characteristic of clinical anxiety.

Study of this phenomenon obviously requires more or less continuous observations over the temporal course of a transaction, permitting examination of the precipitating events and later developments. For this purpose animal experiments are preferable since they are simpler and are not contaminated by *post hoc* verbal rationalizations.

In general the precipitating events have one or other of 3 characteristics. These are: (1) the more or less sudden arrival of information of environmental response inconsistent with the information the anticipatory behavior is, as it were, prepared for; (2) arrival of information previously associated with situations inevitably (*i.e.* regardless of any performance the animal is capable of) including "pain" or "doom"; and (3) change in rate or in variability of information such

that the interacting system becomes overloaded and functionally disorganized.

The initial responses to the precipitating events would appear to include increased activity in one or another of the mesencephalic and diencephalic mechanisms which either facilitate alertness and capacity for rapid change in anticipatory "set," or which provide somatic, particularly visceral, support for patterns of interaction requiring high energy output, such as attack, flight, etc. It is now quite clear that there is a reciprocal interaction between these "alarm" and "emergency" systems and the anticipatory, information processing mechanisms of the forebrain. The anatomical substrata are in part included in the reticulo-thalamo-cortical ascending system of tracts and in the descending cortical connections to the tectum, the reticular formation, and the subthalamic centers, as well as the descending tracts from the limbic lobe. Activity in certain of the last named is apparently capable of inhibiting parts of the alerting and emergency-action functions. It may be noted here that the variety of innate patterns of behavior mediated by the brainstem is such that the general concept of a diffuse mesencephalic-posterior hypothalamic "emergency," "alerting," or "activating" mechanism must be recognized as only a first approximation. Indeed, these aspects of the activity of the systems involved may be secondary functions supportive to relatively precisely integrated behavior patterns.

The later behavior following the more or less acute change in mode of transaction shows wide range and considerable modifiability. The only common characteristic of the numerous patterns seen would appear to be that they result in *decrease in the area of interaction*—both spatial and temporal—between the organism and the environment. This appears to be accomplished either by reduction of the motility of the organism or by limitation of the scanning activity of the information processing systems.

It is important to note that the careful, quantitative studies of animal behavior in experimentally limited situations and with attention to the time axis permit differentiation of classes of behavior not readily ap-

parent in field or clinical observations. In particular, the sharp differentiation of two types of "learning," one abolished and the other virtually unaffected by electroconvulsive stimulation and also by reserpine, represents an important contribution not only to learning theory but also to knowledge of the anxiety-type of phenomena. Concurrent studies of physiological, endocrinological, and psychological phenomena promise further differentiation both of patterns of disturbances of anticipatory behavior and also of the control mechanisms which may be sequentially brought into play.

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## PSYCHOSOMATIC APPROACH TO ANXIETY<sup>1</sup>

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Psychosomatic studies on anxiety and other affects such as anger have been conducted mostly in humans under the influence of psychoanalytic concepts. The earliest Freudian notions conceived of anxiety as a derivative of repressed libidinal drives; the later metapsychological ideas formulated anxiety as a signal within the ego, warning of danger from the pressure of unacceptable internal attitudes. Thus anxiety initiates or intensifies repression and reaction formation against the drives, utilizing their energy for this purpose. Freud considered that the physiological excitation in the cardiorespiratory systems at birth is the prototype of the perceived unpleasurable somatic discharges accompanying anxiety and that anxiety as a signal represents a primitive call for help by the helpless infant.

Implicit in all this is the central theory of a mobile psychic energy which, when obstructed from direct expression by repression, becomes blocked and creates disturbances in organs that are usurped for substitute outlets. We hear a great deal about physiological disturbances associated with repressed anger, oral dependency, depression, and even anxiety itself. In fact, anxiety is attributed to patients who have well-developed defenses against expression of what would cause anxiety if it were released through interference or blocking of the repression or inhibition.

It is in the repressed or in the nascent state of "rising to the surface" that unacceptable feelings and attitudes begin to evoke the signal of anxiety and influence psycho-

logical and behavioral processes. Then, defenses may be intensified, loosened, or shaken. New emergency defenses may be adopted, or behavioral flight into patterns of avoidance, or aggressive attack may occur.

In my opinion the concept of unconscious anxiety does not lend itself to the establishment of fruitful psychosomatic hypotheses because we cannot consider a person to be anxious unless he consciously experiences this unbearable feeling and whatever idiosyncratic accompaniments it provokes. In fact, anxiety appears after considerable degree of psychological maturation and only in humans after adequate self-differentiation and self-orientation with respect to past, present, and future time. In this sense it differs from irritability and vigilance in other animals. Not only are humans conscious of their anxiety, but they are capable of reporting its presence.

Lesser quantities of anxiety are synonymous with alertness or vigilance, constituting states of constant preparation against external dangers but are so constant and automatic that they are accompanied by little psychological cognizance or awareness of somatic participation. Greater quantities of anxiety occur episodically as apprehension under conditions appropriate to the preparation for intensified activity under strain. Then there is cognizance of anxiety and its associated physiological processes such as tachycardia, increased respiratory rate, increased perspiration, tremor, etc., all of which indicate readiness or facilitation of function.

Free anxiety of greater degree, either continuously or in attacks, is neurotic anxiety. It is one of the most unendurable states to which man is subject. Temporarily it may lead to facilitation of psychological and behavioral processes, but in greater amounts it is accompanied by disorganization of functioning and to increasing disturbance leading to regression. At all levels of anxiety there are various degrees of disequilibrium and attempts at re-establishment of stability.

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<sup>2</sup> From the Institute for Psychosomatic and Psychiatric Research and Training of the Michael Reese Hospital, Chicago, Ill. This essay is based on the work of a research group composed of Roy R. Grinker, M.D., David A. Hamburg, M.D., Melvin Sabshin, M.D., Harold Pershy, Ph.D., Sheldon Korchin, Ph.D. and Harold Basowitz, Ph.D. supported by the U.S. Army through the Medical Research and Developmental Board under contract number DA-49-007-MD-469.

It is this latter aspect that sets into action many psychological maneuvers which may be successful in warding off anxiety but results in severe psychopathology. Thus, many of the syndromes to which we attach special names may be viewed as chronic defenses against anxiety. Yet, anxiety in lesser degrees may facilitate growth and development, and functions often to mobilize and intensify the organism's capacity toward a higher level of functioning and new forms of adjustment.

It is our contention that the conscious, painful, current affective state characterized by the term anxiety is but a part of a total organismic process with coordinated and lawfully interrelated somatic, psychological, and behavioral events best conceived of as comprising a transactional field. It is from this frame of reference that the study of anxiety may be reasonably considered as a psychosomatic approach.

Although fear for objects universally accepted as dangerous may be associated with similar psychological and physiological experiences, and there is often difficulty in discriminating anxiety from fear, anxiety is a reaction which signifies an internal meaningfulness to the person experiencing it although the subject tends to attribute this meaning to reality. Within the context of our definition, anxiety as a conscious and reportable experience includes intense dread and foreboding of some objectless or poorly defined future danger.

We have been accustomed to speaking of anxiety as if there were two sharply distinctive categories differentiated by their quantitative factor and whether they are preparatory (signal) or the end-result of a process of declining ego controls and regression (traumatic). The latter is viewed as being disruptive and analogous to infantile physiological reactions, prototypic of birth trauma, which resemble precursors of rage. The global responses of the infant to distress or need are internal emergency homeostatic mechanisms. On the other hand, signal anxiety, which is milder in degree, often facilitating improved functioning, seems to be derived from a cry for help transformed into an activity of self-help. Equilibrium is maintained and defenses tightened as the first signal. Yet, as we view the phenomenon

of anxiety clinically, we see all gradations of increasing amounts of anxiety, associated with decreasing degrees of ego control, from signal, preparation for action, to traumatic.

However, there is some evidence that these two aspects of anxiety are linked to different fundamental biological responses and different psychological concomitants. We refer to guilt- and shame-anxiety. By no means does this exclude the possibilities that a wide variety of different general psychosomatic processes are linked to the monistic phenomena experienced by the subject. Although the sufferer from anxiety cannot spontaneously differentiate its source, since to him it is usually contentless, nevertheless there are means by which we may obtain clues as to the specific source of a particular anxiety.

Anxiety is part of a stress reaction which is set off by a variety of stimuli in which may be included symbolic cues obtained from the external world through interpersonal relationships, or physiological signs that develop from internal somatic disturbances, such as malignancies or other disintegrative diseases. From whatever source, the stimulus leads to a reaction that is synonymous with an organism-in-stress, leading to preparatory processes for emergencies that are not localized but give the feeling of a life-threatening danger. Such neurotic anxiety is rarely continuous except in unusual cases in which considerable ego-regression has already occurred. In its discontinuity, each bout of anxiety seems like an emergency, and the patient rarely learns otherwise, even though he is repeatedly convinced by the end result that "nothing actually happens."

With the anxiety, there are physiological concomitants which often intensify the subjective reaction through a feedback from the perceived physiological effects. Thus, a tachycardia will give to the subject a sensation of heart disease which will augment the anxiety that anteceded and was primarily associated with the tachycardia. We have found that the somatic disturbance which is perceived by the subject reappears in an identical pattern for each individual in recurrent bouts of anxiety, whether they are related to the same stimulus or not. Thus, anxiety has its individual somatic signature

which seems to have been conditioned in early life and maintains its repetitive nature. Except in severe disintegrations, the somatic locus, recognized as being concomitant with anxiety, is limited to a particular bodily function and does not spread to all systems. It is a part reaction of a global infantile response which has become differentiated through life experiences.

Investigations in the field of anxiety are often directed toward special systems. We may isolate them for the purposes of research and focus on each one through special technical devices. The intent of such research would be to determine the effect of anxiety on the functions of a system which is selected because of its empirical relationship to anxiety and its closeness to the central state. If more than one system is used, any attempts at correlation would require simultaneity of observation, since the reactions of any one system in relation to anxiety may take a different time span in its cyclic course and may be determined either by an effect from the central state or by its position in a chain of events emanating from another system. If we assume that anxiety is a psychosomatic process, we may then postulate that many somatic, psychological, and behavioral foci or variables are functions of that process.

Accordingly, research in anxiety from this point of view would be directed toward answering the following broad questions: (1) When the level of free anxiety is altered, what concomitant changes occur in each of the other systems? (2) When the existing level of activity of any given system is influenced experimentally, what concomitant changes, if any, occur in free anxiety? (3) When the existing level of function of any given system is influenced experimentally, what concomitant changes, if any, occur in the other systems in addition to anxiety? (4) When a change occurs in any system, such as anxiety, what are the sequences and relative degrees of change in the other systems?

The title of this essay precludes my discussing the relationship to anxiety of the psychological system as characterized by ego functions of perception and decision, nor does space allow consideration of the general behavioral processes associated with varying

degrees and types of free anxiety. Suffice it to say, there are profound changes in both systems.

In our multidisciplinary researches on anxiety we were also concerned with hypothesizing and then testing the significance of physiological phenomena suggested by clinical experience to be primary variables. We speak of these as being closest to the central process as contrasted with those that are intensified later after activities have, chain-like, arrived at the periphery. For example, we are more concerned with pituitary hormone secretion than with sweat secretion or dilation of the pupil.

In our initial war experiences we observed that long-standing and severe degrees of free anxiety were accompanied by profound generalized metabolic disturbances which aged the soldiers rapidly. The idea that chronic anxiety may be a significant factor in premature generalized or focal aging seemed a reasonable hypothesis and stimulated the hope that, if the mechanisms were known, even though neurotic anxiety is difficult to alleviate, its effects might be blocked.

We were able to isolate an index of free anxiety arising from disturbance of liver function, in that hippuric acid to a significant degree was excreted in higher quantities in persons with free anxiety as contrasted with a much lower excretion in healthy individuals and an even much lower excretion in schizophrenics. We believe that this increase in hippuric acid excretion is a function of a conjugating enzyme system acting on some degradation products, the source of which is still unknown. In later studies, we found that glutathione was probably related to the production of hippuric acid, since it dropped considerably during periods of stress. Furthermore, amino acid production seemed to rise prior to any changes in hippuric acid. In our current studies, not yet completed, we find considerable evidence that 17-hydroxy-corticosterone is elevated in conditions during which free anxiety is intensified. On the other hand, protein bound iodine as an index of thyroid function was not significantly changed.

Patients who are anxiety-prone and made more anxious by experimental stress are studied physiologically with greater difficulty

than with the methods of psychology, psychiatry, and chemistry, indicating the need for experimental studies on infra-human organisms, provided a chronic state could be induced in them analogous to chronic human anxiety.

In measuring the response of multiple variables in correlation with carefully estimated changes in quantity of free anxiety and other affects, we have the impression (no more can be stated at this time) that correlation among system activities, that is levels, trend, and change in variables, is higher the more nearly the organism is in equilibrium. A stress response seems to evoke, with early and minor disturbances at least, a dysequilibrium or decrease in correlatability in system responses. It seems likely that, as the process continues or intensifies with lessened control, greater excursions of responses will revive correlatability.

Stress which sets off anxiety as a signal experienced subjectively cannot be defined independently of the life situation and the subject's response to it. Disturbing an individual with what conceivably and ordinarily could produce anxiety often results in tightening of defenses and little psychosomatic response.

Some of our investigations, however, show that, in conditions of stress, psychological and somatic reactions are not necessarily concomitant because profound biochemical changes may occur before the development of free anxiety and vice versa. Stress responses to a life situation may be set off by symbolic cues of which the subject is not conscious and evoke physiological responses without the experience of anxiety, which only develops when the stress intensifies.

When we attempt to gain perspective on the current knowledge about anxiety, we are aware that various quantities of timing and responses associated with anxiety and the qualitative selection of participatory systems within the early responses enable some grouping into categories of people. They may be viewed spatially as individual populations whose essential differentiating characteristics must be referred back to personality studies. For each group we need to know the past psychological and metabolic experiences leading to predisposition toward

the reactions they present. We need to know the susceptibility to anxiety responses, the types of cues which are significant, the meaning to the person, the defensive capacities against accepting or maintaining the effect of the stimulus, etc. Thus, even if one uses anxiety measured by psychological criteria in psychosomatic studies, one must always refer back to personality, defined by various psychiatric techniques, from interviews to elicit anecdotes of past behavior, to psychoanalysis for evocation of deeper processes. It is the personality criteria that furnishes the characteristics of the life-space significant for patterned reactions to stress.

I think that I have presented sufficient information in this brief essay to indicate that the theoretical concepts concerned in the psychosomatic approach to anxiety and their operational implementation are extremely difficult and complicated. There is very little that is definitive that can as yet be stated regarding the somatic functions related to anxiety and their meaningfulness to the body economy, nor can we say very much indeed about the long-term effects of continued somatic implication in the chronic neurotic anxieties. We may, however, tentatively classify the categories of somatic disturbances in relation to anxiety without consideration of the various types of anxiety which, in turn, may be associated with different patterned somatic responses. In the first place, there are processes that are associated with the emergency state that anxiety signals, all of which seem to be concerned with the preparation for and the maintenance of the bodily economy at the time. These are the perceived effects of anxiety, which, in feeding back, contribute to its intensification, since these are usually conditioned and habitually associated with anxiety. In the second place, there are the homeostatic processes which tend to counteract the emergency defenses in order to maintain the equilibrium of the organism within its physiological limits. These may be said to be the defensive maneuvers which oppose the disintegration of overintense responses. In the third place, there are over-reactive processes, perhaps associated with greater degrees of anxiety known as traumatic or disintegrative, in which the functioning of organ sys-

tems becomes so disturbed that they exceed the limits of health. Finally, there are the long-lasting effects resulting in the exhaustion of function and the slow wearing-out of tissues that are habitually used in the service of activity concomitant with anxiety.

Our task is to attempt to separate patterns of somatic responses as they are correlated with special types of anxiety. Also, we should like to relate the somatic participation in anxiety with the level of economy of the physiological systems. As has been stated before, all of these need to be put in their proper time and quantitative perspec-

tives in relation to the disturbance in such other systems as the psychological or behavioral.

Not only are these researches necessary for the understanding of anxiety as a total organismic response to stimuli that threaten the integrity of the organism, but they are significant in establishing general laws of psychosomatic organization. Studies of anxiety from the psychosomatic approach could offer a rational prototype leading to the understanding of general laws of psychosomatic organization in health and illness and referable to the treatment of all diseases.

## DISCHARGE FROM STATE HOSPITAL IN RELATION TO COMPETENCY<sup>1</sup>

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As psychiatrists, all of us are aware that there is no direct or necessary relationship between hospitalization in the state hospital and competency. The consensus seems to be that these two things should be considered entirely separately. Many patients may need mental hospital care, without suffering an impairment of their competency. My personal estimate is that 75% of all patients admitted to the average mental hospital could be considered as competent. Also, an individual may require a guardian because of incompetency due to a mental disorder, but not require hospitalization in a mental hospital.

Unfortunately this distinction is not readily recognized by lawyers and courts. It is also frequently forgotten by psychiatrists. There are many forces and factors which tend to foster the impression that hospitalization in a mental hospital is equivalent to loss of competency.

In a number of states, the process of commitment or the language of either the statute or the court order makes commitment of a mental patient to a state hospital an automatic adjudication of incompetency. This is true even though guardianship is not provided for at the same time. The specific effect of commitment to a state mental hospital upon an individual's competency will vary from state to state. I have made no effort to determine this for the various states at this particular time, but I would point out that such information as to his own state's practice in this regard should be worthwhile knowledge for any psychiatrist. It is, of course, imperative that the physician on the staff of a state hospital have such knowledge at his disposal.

The status of the patient's competency while in the hospital is of importance, but of much greater interest to the patient is the

effect of his release from the hospital. If the psychiatrist is to be of help in advising and counseling patients who are ready to be released from the hospital, he must have specific information on the subject, particularly as to the practice and procedure in his own state.

The statutes relating to discharge may alter, by the procedure prescribed, the intent of other provisions of law relating to the hospitalized mentally ill patient. The situation in California provides a good example of this(1). The language of the statutes indicates that the commitment procedure is entirely separated from the determination of competency, and that commitment means only that the patient is in need of hospitalization and treatment. The Supreme Court of the state has ruled that the patient's competency is not affected, unless guardianship action has occurred(2).

This approach is, of course, consistent with the enlightened psychiatric viewpoint. However, the sections of the statutes relating to discharge set up a procedure for the restoration of competency! They provide for the issuance of a discharge certificate by the Department of Mental Hygiene(3). A copy of the discharge certificate may be filed with the clerk of the court of the county from which the patient was committed. If the certificate of discharge indicates recovery, the filing has the same legal force and effect as a judgment of restoration to capacity. If the certificate does not indicate recovery, it does not have this effect.

I shall discuss the legal implication of the terms "improved" and "recovered" later. The point to be made here is that these sections clearly imply that commitment is equivalent to an adjudication of incompetency, notwithstanding the previous sections and the court decision. In actual practice, this situation becomes important in those instances when discharged patients are involved in transactions concerning real estate. If a certificate of recovery has not been filed, then the formal court action of judgment

<sup>1</sup> Read at the 112th annual meeting of The American Psychiatric Association, Chicago, Ill., April 30-May 4, 1956.

<sup>2</sup> Address: Cleveland Regional Treatment Center, 124th and Fairhill Road, Cleveland 20, O.

in restoration to capacity is insisted upon before the discharged mental patient can convey title to real property. It is my impression that a similar situation exists in other states, in regard to certification of recovery upon discharge acting as the equivalent to the restoration to capacity. In at least 3 states, to my knowledge, there is a legal distinction as to the terms "recovered" and "improved." The term "recovered" is regarded as being the legal equivalent to "sane" or "restored to capacity" or competent. The term "improved" is any condition short of this and carries the implication of lack of competency.

However, in determining the condition at time of discharge, the evaluation of the patient is a medical and psychiatric one, rather than on the basis of the patient's legal status or competency. The evalution involves some knowledge of the patient's life situation prior to his illness. The *APA Statistical Manual* has 2 pertinent statements on this subject:

1. "The individual's pre-illness capacity, in terms of occupational and social adjustment, will be used as a base line for estimating the degree of impairment." Note that the term capacity here is not the capacity or competency commonly used in legal terminology.

2. The term "no impairment" or "recovered" as described in the *Manual* is as follows: "This term will be used whenever there are no medical reasons for changing employment or life situation."

Another favored psychiatric term, "remission"—meaning abatement of the symptoms of disease, but implying that the basic disease process is still present—is not used in the *Manual* in this particular section. State hospital psychiatrists are rather conservative in discharge evaluation and the term "improved" is favored over "recovered," even though the patient may have returned to his former social situation and employment. This is especially true since the adoption of the present revision of the nomenclature and the issuance of the *Manual* in 1952. Under this system the diagnosis includes the complementary evaluation elements of (1) external precipitating stress (2) premorbid personality and disposition; (3) degree of psychiatric impairment. There is a tendency

to include the premorbid personality and disposition in the consideration and evaluation of discharge condition on the basis of a permanent defect caused by the psychiatric disorder, rather than to use it as part of the base line for estimating whether the patient has returned to his pre-illness condition.

The essential point that I should like to emphasize is that the evaluation of the hospitalized patient at the time of discharge is a medical and psychiatric one with little or no consideration as to his legal status in regard to competency. Unfortunately, however, in many instances his legal rights are involved in this procedure with injustice and inconvenience to the patient. For the psychiatrists, it should be explained that this situation occurs because of the principle of law involved in "presumption." This means that certain conditions are presumed to exist, and do exist, in the legal sense until it has been affirmatively demonstrated that such is not the case. Thus, for the ordinary adult, there is a presumption in law that he is competent. The burden of the proof is on the person who would have him declared incompetent. However, once he has been determined to be incompetent, this condition is presumed to exist until there has been a judicial determination to the contrary.

If commitment has been ordered and a judicial finding of insanity has been made, mere discharge from a state hospital will not erase or vacate this finding. The law presumes that once an abnormal mental condition has been proved to exist it continues until the contrary is established. The best evidence of such a change is a judicial determination of recovery or discharge. The absence of such a positive judicial finding puts the patient at a disadvantage. In the case of the patient who is discharged as "recovered" with notification to the court and a change in the official record, competency is restored. However, in the case of the patient who is discharged as only "improved," or in which the hospital does not notify the court, the presumption of insanity continues, even though the patient may return home, resume his business, vote, drive a car, and conduct himself just as he did before hospitalization. Should a contract that he subsequently makes be attacked, or the question

of his competency be raised, the burden of proof has now shifted to him to demonstrate his competency.

As previously referred to, many discharged hospital patients become aware of this situation only when they attempt to convey title to real estate. It should be clear that this situation does not apply in those cases in which the patient is admitted on a voluntary basis, or on medical certification, in which no judicial procedure has occurred. However, the injustice is even more apparent if a diagnosis of without psychosis is arrived at and the patient is discharged without notification to the court. It should be repeated that the presumption can be destroyed only by positive judicial action or change in the records.

I would like to make 2 recommendations to psychiatrists on state hospital staffs who are considering patients for discharge:

1. I believe that a more liberal attitude in relation to the use of the term recovered is justified. One of the devices which has been used by our staff is an effort on the part of the psychiatrists to determine the competency of the individual patient at the time he is being considered for discharge. If the patient is regarded as competent and able to manage his own affairs he is classified as recovered for the purpose of the discharge record. The opinion of the staff is expressed both as to the patient's ability to manage his own affairs and his need for future care or confinement.

It should be pointed out here that competency is a legal term; the criteria for competency to make a will are different than for operating a business. A person may be sufficiently competent to purchase food and clothing and to meet simple responsibilities but not sufficiently competent to manage a complex business. A senile person who has managed a complex business for a number of years may be competent to continue managing it, more or less out of habit, although an equally senile person who has never had a similar experience would not be so competent. Each case must be determined individually on the basis of the individual's mental condition and the circumstances of the

situation in which he will be placed. Our psychiatrists have had to enlarge their knowledge of the criteria of competency.

2. I feel that you as a practicing psychiatrist or a psychiatrist on the staff of a state hospital should be informed as to the statutes and the procedures in your own state with particular reference to those governing the discharge of mental patients. You should consult a lawyer for this information or get a legal opinion. Usually in the case of state hospitals this can be obtained from the attorney general's office in your state. If you find that commitment by the court is the equivalent to a determination of incompetency in your state, you should establish a procedure by which you notify the committing court of the discharge of the patient and of his status on discharge. This is particularly important in those cases discharged as recovered, or who have been diagnosed and discharged as without psychosis.

Obviously, these recommendations do not mean that efforts should not be made to change the statutes so that this injustice to patients can be corrected.

I would point out that the language of the Draft Act governing the hospitalization of the mentally ill(4), in referring to hospitalization upon court order, specially limits the findings in regard to capacity to "Lack of sufficient insight or capacity to make responsible decisions with respect to his hospitalization." Likewise, under Section 21 of the Draft Act, it is specifically indicated that every patient retains his civil rights unless he has been adjudicated incompetent and has not been restored to legal capacity.

It would be interesting to know what the experience of those states which have adopted the Draft Act has been. Unfortunately only a few states have thus far seen fit to wipe out their antiquated statutes and adopt it.

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## SEMINARS FOR EXECUTIVES AND INDUSTRIAL PHYSICIANS

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The Division of Industrial Mental Health of The Menninger Foundation undertook 2 experimental seminars, 1 for executives and the other for industrial physicians, in an effort to learn how psychiatric concepts might most fruitfully be communicated to these 2 groups and what value such communication might have.

The seminar for executives, "Toward Understanding Men," was held January 30 to February 4, 1956. Participation was limited to 20 executives whose decision-making or leadership responsibilities were such as to have a significant impact on the morale or emotional climate of an important segment of a company. Objectives of this seminar were to help the executives further their understanding of human motivation, sharpen their interviewing and communication skills, and enhance their effectiveness in the supervision of others.

These objectives were based on the assumption that the major task of the executive is to be concerned with people—their growth and development within the organization; their morale and motivation; their conflicts and problems which relate to the job; their efficiency on the job; their working relationships with each other; and the complementary identification of employee and company with their mutual goals and purposes.

The seminar was divided into 4 major units: (1) psychodynamics or basic theory of psychological motivation; (2) interpersonal communication; (3) psychological factors in supervision; (4) emotional problems of executives.

Three half-day sessions were devoted to each of the first 3 subjects. Included in the unit on psychodynamics was a discussion of conscious and unconscious motivation, another on constructive and destructive personality trends, and another on personality mechanisms. The unit on interpersonal communication included a discussion of principles and techniques of interviewing, a second on

developing group participation and a discussion of cases and problems in communication submitted by the participants. The third unit on psychological factors in supervision was devoted to the psychological role of the supervisor, counseling in supervision, and cases and problems in supervision submitted by the participants. The fourth unit was a lecture and discussion of emotional problems of executives.

The method of presentation was an hour-long lecture, followed by a half-hour question-and-answer period with the lecturer. This, in turn, was followed by a group discussion. The 20 participants were divided into 2 groups of 10 each with a discussion leader, and discussion was focused on the lecture and relevant questions. In addition to discussion in the groups, there was opportunity for role-playing and other forms of demonstration.

The first 3 lectures were given by psychiatrists and were based on psychoanalytic ideas, made as specific and simplified as possible.

The second unit dealt primarily with social work concepts. It was felt that the principles and methods of interviewing as used by social workers would be more useful to executives than much psychiatric interviewing would be. The same was felt to be true of developing group participation. The third lecture in this unit, that on cases and problems in communication, was given by a psychiatrist who reviewed all of the cases and problems submitted in advance by the participants, and illustrated how some of these problems would be viewed by psychiatrists.

The psychological role of the supervisor was a discussion of the father-figure aspects of supervision and the expectations on the part of those supervised. The session on counseling in supervision was again built around the social work practice in supervision which was felt to have parallels in the industrial practice of executive development. In the third session a psychiatrist reviewed

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all of the cases and problems submitted which related to supervision and illustrated what meaning these might have from a clinical point of view.

The executive group was initially somewhat skeptical of the emphasis placed on the early development of personality and was much more concerned with "how-to-do-it" specifics which could be applied on the job rather than with general concepts. However, as the week went on these resistances diminished and the group then became much less interested in specifics. In fact, toward the end of the week the participants were more concerned with obtaining a general point of view and found attempts to talk about specific problems less valuable than an over-all perspective.

The group was an extremely interested one, asked many questions, and participated actively in the discussions. Their extraordinary interest surprised the faculty and made it obvious that the presentation of a psychiatric point of view was something which this group not only wanted, but also permitted the participants to consolidate and integrate many of the things which they had learned in a haphazard and sporadic way before. As one man put it in the evaluations, "This has been a remarkable re-direction of my thinking and a relating of the majority of the previously separated areas of my life."

Those aspects of the course which related to specifics were the least successful. The men felt that interviewing was something which had been long available to them through other kinds of management seminars and need not have been duplicated here. They also found difficulty in bridging the gap between some of the examples which were offered, as in social work, and their own day-to-day activities.

They were most reassured in the discussions of supervision and communication when they learned that they would be most helpful if they themselves were not to try to go beyond the ordinary means of communication and supervision available to them to help persons who were having problems, but should refer these persons to those professionally skilled in dealing with such problems.

In its evaluation session the group spon-

taneously voted to have a follow-up session 6 months later for 2 or 3 days in which they would try to assess together what this week-long experience had meant, after having been back on the job a while. Subsequent communications have indicated that in various ways participation in the seminar was directly helpful to a number of the participants in their every day on-the-job activities.

This session demonstrated, to the faculty's satisfaction at least, that psychiatric concepts can be communicated to lay people in a useful way without suggesting or implying that these people are to become therapists. In fact, it was most reassuring to them to learn that many interpersonal problems could be understood a little better if looked at from a psychiatric point of view but that the actual application of psychiatry was best left to psychiatrists.

The second 1-week seminar was conducted along much the same lines for industrial physicians. Participation was limited to 20 physicians and 2 alternates, all members of the Industrial Medical Association.

The seminar, "Maintaining Emotional Health," was directed to industrial physicians who were concerned with the health maintenance of a major segment of a company. It was expected that it would be particularly helpful to those physicians who recognized the need for orientation to the practical application of psychiatric knowledge and skills in their daily clinical activities.

The objectives of this seminar were to help the industrial physician further his understanding of psychological motivation; improve his skills in assessing the emotional aspects of medical problems; familiarize himself with the techniques of brief psychotherapy; enlarge his knowledge of psychiatric treatment methods, criteria for referral, and processes of rehabilitation; and develop a systematic point of view with respect to emotional influences on health.

These objectives were based on the assumption that the major responsibility of the physician in industry is to maintain health. It was assumed further that the industrial physician takes a holistic point of view and therefore is concerned with feelings and attitudes in the relationship to health, as well

as with organic conditions, the plant environment, and physical stresses.

The first unit of this seminar was essentially the same but at a somewhat deeper level than that for the executives. After a discussion of conscious and unconscious motivation, constructive and destructive personality trends and personality mechanisms, these were tied together in a psychosomatic case presentation.

The second major unit was principles of psychiatric examination. Here there was a discussion in 3 sessions of the doctor-patient relationship, brief screening methods, and psychiatric case study. In the first 2 of these sessions, the participants were the guests of Winter VA Hospital where, following the lecture on each subject and the opportunity to question the lecturer, the physicians observed demonstrations of psychiatric interviewing through a 1-way vision screen. Following the demonstration they were able to discuss with the demonstrator particular issues in the interview and points which the demonstrator was trying to make. While the opportunity to view such interviews was well received, it would have been even more helpful had there been someone behind the 1-way screen with them, equipped with a microphone, to describe to them particularly important points as the interviews were going on. The third lecture was given at the Topeka State Hospital where a detailed case presentation was worked up and discussed with the participants.

The next unit, psychiatric treatment, was devoted to somatic treatment methods, rehabilitation of a psychiatric patient and brief psychotherapy for the industrial physician. Describing somatic treatment methods and rehabilitation problems of a psychiatric patient, the focus was not on such treatment as these men themselves might give, but rather on the implications of somatic treatment methods for a person returning to work, and further, on the problems, as seen from the hospital point of view, that a patient encountered when he returned to work and with which the industrial physician could help him. There were 2 lectures on brief psychotherapy, one on principles and the other on practices. In the latter, an effort was made to communicate specific techniques

of brief psychotherapy which would be useful to the industrial physician. The experience of the week provided an excellent setting for the integration of the necessarily elementary techniques of psychotherapy. What was being said about "how to" had specific meaning in the context of all that had gone before.

There were 3 evening sessions in the medical seminar. The first was on the scope and limitations of psychological tests. The industrial physicians were very much interested in this subject because of the reported increasing use of psychological tests in industry. They were concerned about the usefulness, the effectiveness, and the contribution which tests could make to the work of the medical department. They were also much concerned with instances of what they viewed as indiscriminate use of tests, and the anxiety-creating effects of inadequate interpretation to employees. The second evening session was devoted to problems of neurology and neurosurgery in industry and the third to a presentation of the VA film, "Psychiatric Interviewing: Method and Procedure," with a discussion afterward.

In discussing problems of occupational rehabilitation of the psychiatric patient and referral to psychiatrists, the industrial physicians complained that often they were unable to get any information from psychiatrists about the patients. They said that they were less concerned with diagnoses than with learning from the psychiatrist his specific recommendations about how the patient should be reassigned, followed, and helped. This discussion gave rise to the suggestion that a committee from the Industrial Medical Association should meet jointly with the Committee on Industrial Psychiatry of The American Psychiatric Association to formulate a set of principles which would guide both industrial physicians and psychiatrists in their relationships with each other.

The seminar participants apparently found the seminar quite successful. Initially they had perhaps expected to be "talked down to" and to be overwhelmed with jargon. They found, however, that the faculty was just as interested in learning from them about the problems with which they had to deal as they were in learning from the faculty. Again

the enthusiastic interest of the group stimulated the faculty and demonstrated to them that they need not be hesitant about trying to communicate to their professional colleagues the point of view with which they work and some of the elementary principles which they use in daily practice.

An added result of the meeting was the stimulus, as expressed by some of the seminar participants, to effect a closer liaison with psychiatrists in their own communities and to introduce more psychiatric teaching into academic departments of industrial medicine. There was considerable discussion about how to use psychiatrists and clinical psychologists in industry. Several of the participants reported that their company medical departments were considering the employment of a psychiatrist either on a consulting or part-time basis.

We were, of course, gratified by the re-

sponse of the participants and with the large numbers of requests for participation—far more than we could meet. Yet, though we were able to meet on common ground with the participants in both groups and able to feel that we could in a modest way make a small contribution to them, we could not help being impressed with the fact that many of the problems with which they have to deal call for a knowledge and understanding far beyond our present limits. These meetings were a challenge to us to increase our efforts to learn more, not only about the kinds of problems with which these men have to deal, but also about effective ways of dealing with them. It is our hope that many psychiatrists across the country will interest themselves in the problems of industrial mental health so that we may soon begin to collect a body of experience and research data which will permit us to make a meaningful contribution to the emotional health of people at work.

## MOOT QUESTIONS IN PSYCHIATRIC ETHICS<sup>1</sup>

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Medicine is a science blessed with the magnificent heritage of many great men. Men such as Pasteur, Osler, and Harvey are known to lay people as well as to doctors. Medicine is the concern of everyone. It is our opinion that of all the men who have contributed to medicine, no one has been more intimately reflected in its actual practice than Hippocrates.

Hippocrates, who lived over 2,000 years ago, is considered the Father of Medicine. He initiated many fine principles, including the scientific method (1), but none has surpassed his ethical values recorded in his well-known Oath, which from its inception has been the basis for medical ethics. The basic principles have remained relatively unchanged. When one considers that there is no procedure in medicine that can be performed without ethics being involved, one can understand the prominent place that this Oath has attained. Dr. Robert I. Lee (2), in an address before the World Medical Association, stated:

The Hippocratic Oath is entirely unselfish and altruistic. It relates to the behavior of doctors for the benefit, not of doctors, but for the benefit of patients. It has been a touch of idealism in a selfish world.

In America, the American Medical Association has re-studied, revised, and enlarged upon the ethical principles of the profession 4 times since 1902. In general, it outlines ethical considerations of the character of the physician, his duties and responsibilities to his patients, to his profession, to his colleagues, and to the general public.

The first sentence of the preamble (3) states:

These principles are intended to serve the physician as a guide to ethical conduct as he strives to accomplish his prime purpose of serving the common good and improving the health of mankind.

<sup>1</sup> Read at the 112th annual meeting of The American Psychiatric Association, Chicago, Ill., April 30-May 4, 1956.

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In considering the application of ethical principles to the practice of medicine, it should be pointed out that there are many situations where ethical behavior is clear and definite. The physician is either ethical or he is not. For example, if a psychiatrist commits a patient to a mental hospital when he knows the patient is not committable, but does so for financial gain, it is obvious that he is practicing unethical medicine. When, however, as Sperry (4) points out, you have two principles, two forces which are both valid and right, but cannot exist simultaneously, and "the claim of each is equally justified, but the right of each is pushed into a wrong because it ignores the right of the other", you have a dilemma demanding ethical action. Thus, if the rights of an individual patient are in opposition to the rights of society, the doctor or the psychiatrist is confronted with the necessity of making a difficult decision which intimately involves his ethical approach to the practice of medicine. In this area, there are considerations about which there is a good deal of uncertainty and divergence of opinion. It is the purpose of this presentation to attempt to obtain better understanding about these doubtful points. To learn more about ethical considerations, we sent the following questionnaire to 67 of our colleagues in Philadelphia, most of them our associates at the Institute of the Pennsylvania Hospital:

Dear Doctor:

Doctor Ralph Little and I are interested in learning more about the less commonly recognized aspects of medical ethics, and have listed some situations that we feel many of us have faced. We would like to have your answers to the questions we have proposed in any way you care to answer them. You might feel that these questions should be answered by a lawyer, but in our experience in this area, psychiatric thinking is more advanced. Thus, we would like the answers to come directly from you. It is important to remember that your opinions, more than anything, determine what are our medical ethics, and that is why we feel your opinions are so important. We will not quote you in any way. Thank you.

EDWARD A. STRECKER, M.D.

(1) What is the ethical responsibility when you have definite proof that a colleague of yours, who is practicing psychiatry is: (a) a narcotic addict; (b) psychotic; (c) indulging in sexual activities with his patients; or (d) prescribing narcotics to addicts for self-administration, which is illegal in that state?

(a) What is your ethical responsibility to a minor whom you are treating, when you definitely believe this patient is homicidal and/or suicidal, and the responsible parent or guardian will not attempt to take measures to protect your patient or society?

(3) What is your ethical responsibility when a patient relates to you he has committed murder, and then forbids you to notify the police on the basis his communications to you are secret?

(4) What is your ethical responsibility when a patient tells you her husband is ill and is planning to kill her, and you have reason to believe this is true, but she will not permit commitment or help from the police, and forbids you to interfere?

(5) What is your ethical responsibility when one of your unmarried patients becomes pregnant during treatment and informs you that she has arranged to have a criminal abortion? There are no indications for a therapeutic abortion, and she forbids you to divulge her professional confidence.

(6) What is your ethical responsibility when a patient whom you are treating for narcotic addiction is securing the drug from illegal sources?

(7) What is your ethical responsibility when you know one of your patients is embezzling, and he refuses to do anything about it?

(8) What is your ethical responsibility when one of your patients takes out a large insurance policy, and plans to commit suicide as soon as the suicide clause is fulfilled, and he does not accept your recommendations for treatment?

(9) What is your ethical responsibility when a patient whom you have had in treatment for some time reveals acts of disloyalty and sabotage against the United States, and demands that you keep such information secret by his right of privileged communication?

We received 42 responses, but, in tabulating them, could use only 38 as 4 were too general.

It should be emphasized that the above questions were not a product of our own imaginations, but had occurred in our practice and in those of our colleagues. In tabulating the replies, whenever a psychiatrist gave more than one choice, we recorded his first choice.

To Question 1(a), "What is your ethical responsibility when you have definite proof that a colleague of yours, who is practicing psychiatry, is a drug addict," there was almost unanimous opinion—that is, 36 felt that some action seemed indicated in this situa-

tion. Two psychiatrists felt they would take no action unless there was definite proof that the addiction caused the doctor to be negligent in his duties. However, there was a difference of opinion as to what measures should be taken. Sixteen of the 36 who felt they should do something about it thought they would try to help the physician by consulting with him directly. Sixteen indicated their first action would be to consult a committee on ethical procedures of their organized group, so that it might take the responsibility. One would notify his family, and 3 would consult with a senior psychiatrist.

In part (b) of the first question, "What is your ethical responsibility when one of your colleagues is psychotic?" there was a unanimous feeling that action should be taken, but again there was divergence about what should be done. Eighteen recommended consultation with an appropriate committee or authorities, 14 would consult the doctor personally. Of the remainder, 3 recommended consultation with a senior colleague, 2 physicians would notify his family first, and one would take whatever steps necessary to achieve a solution.

To Question 1 (c), "What is your ethical responsibility when one of your colleagues is indulging in sexual activities with his patients?" 32 doctors indicated they would do something and 18 of these would recommend consulting with a committee, while 9 would talk with the doctor personally. Two would consult with a colleague, and 3 would try to help the doctor get treatment. Of the 6 who indicated they felt the ethical responsibility did not require action, one said he would do something if the individual doctor was psychotic.

To the fourth part of the first question, "What is your ethical responsibility when a colleague of yours is prescribing narcotics to addicts for their own self-administration which is illegal in that state?" all but 4 felt definite action was indicated, and 23 of the 34 would seek help from an appropriate committee or authorities. Only 7 would consult directly with the doctor; 3 would take whatever steps necessary to solve the problem; and 1 would consult with a senior colleague.

Question 2.—"What is your ethical re-

sponsibility to a minor whom you are treating, when you definitely believe this patient is homicidal and/or suicidal, and the responsible parent or guardian refuses to take measures to protect your patient or society?" In response to this, 29 indicated they would like some definite action and would inform the proper authorities such as the police, district attorney, courts, school authorities, or the appropriate social organization. Of the 9 who would not inform outside authorities, 4 felt they would stop treatment of the minor after informing the family; 2 would seek consultations, and 3 would continue treatment.

Question 3.—"What is your ethical responsibility when a patient relates to you that he has committed murder, and then forbids you to notify the police on the basis that his communications to you are secret?" Thirty felt they would deny the patient's request for privileged communication, and do something. Of this group, 18 would report this action to proper authorities such as the police or district attorney. Five of the 30 indicated they would use their own judgment about informing the authorities, and this would depend on the nature of the murder and how long ago it had been committed. The remaining 7 would either consult with the local society, a relative, colleague, lawyer, or stop treatment if the patient would not turn himself in.

Of 8 who felt no action was necessary, 1 stated very definitely that there was no legal responsibility, and 7 felt that they should continue treatment, the responsibility of reporting the crime being up to the patient.

Question 4.—"What is your ethical responsibility when a patient tells you her husband is mentally ill and is planning to kill her, and you have reason to believe this is true, but she will not permit commitment or help from the police, and forbids you to interfere?" Twenty-five individuals expressed responsibility for taking some action. Of these, 12 would report to legal authorities; 7 would attempt to force commitment either of the husband or the wife, some feeling that the wife might be suicidal. Five would consult with a colleague, and 1 would refuse treatment if the patient would not accept help. Thirteen indicated they felt their only responsibility would be to continue to treat the

patient, trying to point out the meaning of the patient's behavior to her.

Question 5.—"What is your ethical responsibility when one of your unmarried patients becomes pregnant during treatment and informs you that she has arranged to have a criminal abortion?" There are no indications for a therapeutic abortion, and she forbids you to divulge her professional confidence. In this question of ethical responsibility, 29 indicated they would take no definite action, and most of these stated they would try to help the patient understand its psychological significance. (Four did not feel ethical responsibility was involved, and 1 felt there were too many variables to answer this question.) The opinions of the 9 who felt their ethical responsibility demanded some action varied greatly. One would try to commit the patient if she were committable. One would try to find the name of the abortionist and report him anonymously; 2 would stop treatment if the patient insisted on the abortion. Of those who would report this, 1 would report to the district attorney, 1 to a public health officer, 1 to the parents, and 1 to the medical society. One felt that there were too many variables to answer the question.

Question 6.—"What is your ethical responsibility when a patient whom you are treating for narcotic addiction is securing the drug from illegal sources?" In reply to this, 22 felt they would do something in this situation. Thirteen would report this to the proper law enforcement authorities. Of this group, 3 would report and refuse treatment, 3 would try to hospitalize the patient and then report, and 7 would just report it. The remainder who would take action indicated that 3 would only stop treatment, and 2 would report it to the appropriate committee. Four would take whatever means necessary to hospitalize the patient. Of the 16 who would not report this, 13 indicated they would continue treatment, and 3 stated they felt they had no ethical responsibility.

Question 7.—"What is your ethical responsibility when you know one of your patients is embezzling, and he refuses to do anything about it?" Response to Question 7 revealed that 22 would not feel it their ethical responsibility to report this patient. One

would, however, report only if the patient were paying his fees from the embezzled money. Two made the definite statement that no responsibility was present, and 3 were noncommittal. Sixteen would continue treatment. Of the 16 who would "do something," 6 would stop treatment if the patient refused to report himself. Six more would only report him, either to his family, to his employer, or to the medical society. Two felt that the amount of money would be the determining factor of whether or not they would report him. One would try to commit him to a hospital, and 1 would report him to the local medical society.

Question 8.—"What is your ethical responsibility when one of your patients takes out a large insurance policy, and plans to suicide as soon as the suicide clause is fulfilled, and he does not accept your recommendations for treatment?" In this question of responsibility, 18 felt there was no responsibility to do any more than to treat the patient without breaking his confidence, and 5 of this group specified they would urge hospitalization. Twenty also felt that their responsibility demanded more than to continue treatment; 10 would continue treatment, but inform the family; 2 would insist on commitment; 2 would report it; 1 would call in a consultant; another would consult with the medical society. Three would inform the insurance company, 1 only if he committed suicide. One would stop treatment.

Question 9.—"What is your ethical responsibility when a patient whom you have had in treatment for some time reveals acts of disloyalty and sabotage against the United States, and demands that you keep such information secret by his right of privileged communication?" In response to this question, all but 3 indicated they felt there was ethical responsibility to report such information. Of these 34 (one wished his response would not be used), 27 would report to the proper authorities, and of these, 13 indicated that they would have to evaluate personally the seriousness of the acts before they would report them. Two would consult their lawyers; 2 would refuse treatment and seek medical consultation; 2 would report to the

medical society. One was undecided what he would do.

We would like to discuss our observations from this questionnaire with the following considerations:

We felt that the responses pointed out certain doubtful areas in the concept of privileged communication, by which we mean that the patient expects what he tells his doctor to be held in confidence. This is primarily, but not exclusively, an ethical consideration, for the right of privileged communication means that the doctor has the privilege of deciding whether or not he will reveal to others what is said to him in confidence. We understand that there are 17 states which retain the common law rule under which no privilege is recognized covering communications between patient and physician. Other states respect the privileged communication, and consider the doctor liable when he reveals such information to a third party(5).

Of all the specialties, psychiatry has to depend more than others on the idea of secrecy to be effective therapeutically. We do not see how psychiatrists could successfully treat patients with emotional problems if they cannot say to them, in effect, "Tell me about all your thoughts and feelings—no matter what they are. You don't have to worry about my telling anyone about them." This concept must be part of the very basic structure of the doctor-patient relationship. Hippocrates thought so, and so wrote in his famous oath: *Whatsoever in the course of practice I see or hear (or even outside my practice in social intercourse) that ought never to be published abroad, I will not divulge, but consider such things to be holy secrets.*

The AMA enlarged upon this in Chapter II, section 2, of the *Principles of Medical Ethics* of the AMA:

Confidences concerning individual or domestic life entrusted by patients to a physician and defects in the disposition or character of patients observed during medical attendance should never be revealed unless their revelation is required by the laws of the state. Sometimes, however, a physician must determine whether his duty to society requires him to employ knowledge, obtained through confidences entrusted to him as a physician, to protect a healthy person against a communicable disease to which he is about to be exposed. In such instance, the physician should act as he would desire another to act toward one of his own family in like circumstances. Before he determines his course, the

physician should know the civil law of his commonwealth concerning privileged communications (3).

Thus, both Hippocrates and the American Medical Association seal the lips of the physician unless there is something he feels he "ought" to reveal. Therefore, when the psychiatrist learns something which, if not revealed, might cause innocent people to suffer, he may be forced into a role for which he has not had proper training. The psychiatrist may have to assume the role of a judge, without the wisdom and experience of a judge, in trying to carry out his ethical responsibilities. He may be placed in the tragic situation of trying to serve two loyalties which appear to be in conflict with each other; namely, his loyalty to the practice of psychiatry, with its allegiance to the secrecy inherent in the doctor-patient relationship, and his loyalty as an American citizen to his country and its laws. We have no way of determining how frequently this dilemma may occur in psychiatric practice, but we feel the chances are good that many psychiatrists may be faced with such problems some time during their psychiatric experience.

In making his decision, the psychiatrist will have to decide for himself what information he considers he should or should not reveal. He may differ with a patient who feels that certain information should be regarded as privileged, and may feel that the patient has no right to expect him to conceal certain information which, if kept secret, would result in harm to others. He may feel that by withholding such information he may indirectly be giving sanction to harmful behavior. His decision about his ethical responsibility will depend on his own personality and experience. There may be times when his ego, that part of his personality that deals mainly with the realistic aspect of the entire problem, will predominantly determine his actions. At other times, his superego, that facet of the mind which is closely allied to his conscience, may take the leading role in arriving at his ethical actions. In coming to a decision, we feel it is important to remember what John W. Reed, of the University of Michigan, wrote to a doctor about this consideration:

It appears that a psychiatrist is under no legal duty to report to law enforcement officials a confession

of crime made by a patient in the course of professional treatment. If he does report, he runs the clear risk of a lawsuit at the instance of the patient, there being in the few decisions on the subject a continuing assumption by the courts that there is a civil liability for *unwarranted* breach of confidence. Should such a lawsuit occur, the psychiatrist could apparently prevail over the patient by showing that the disclosure was for the purpose of guarding the community against the possibility of further harm at the hands of the patient.

We would like to discuss the divergence of opinions we received to this questionnaire. We have felt that all the men who received it had the highest ethical responsibility, and for anyone who might question this, the burden of proof would most decidedly be on him. It might be felt that we did not have a fair cross section of psychiatric opinion, for we had responses of men primarily from a certain area of Philadelphia. This may be true, but we are proud of our psychiatry in Philadelphia, and of our 5 medical schools (we had representatives of each in our responses), and we do not feel that we are out of touch with the best psychiatric opinion in the country. Certainly, we do not know of any registered complaints against our ethics. Another objection could possibly be that we might have had different responses if our study had demanded that the responses be anonymous. We can point out that some did send in anonymous replies.

We did not feel that because some of our colleagues believed it their ethical responsibility to do something about some of these matters, while others felt they had no responsibility, that some were ethical and others were not—just as one is not right and the other is not wrong.

These responses did indicate that there are moot questions in psychiatric ethics. The reason for the diversity of opinion seemed to indicate to us a commonly understood fact that each one's own ethical activity is determined by his personality and experience. There are, of course, advantages and disadvantages in this, and it is our hope that by further discussion of the uncertain areas of ethical conduct, a greater understanding of our responsibility will ensue.

The advantages are, we feel, centered around the idea of freedom. Medicine continues to make rapid advances, the benefits

of which will most likely have some effect directly or indirectly on all of us. Progress in the lessening of human suffering has been accomplished in an atmosphere of freedom which the physician respects and feels he needs for his work. Because a physician works in an area of freedom, his ethical behavior will reflect this, and it would seem deplorable if any attempt were to be made which would legislate his activities, for he is very jealous of his freedom and has certainly put it to good use.

The disadvantages of divergences of ethical responsibilities in psychiatry are that some individual will possibly suffer because of this. We can only hope that this suffering is minimal. We want to repeat that no procedure in medicine can be carried out without ethics being involved, and this emphasizes the importance of further enlightenment on the subject.

We would like to be so bold as to make some recommendations from the observations we have drawn from this study. There seemed to be a feeling that it would be helpful if there were an *active* committee on ethics in the local medical societies to whom doctors could go with their problems. Such a committee would be available to psychiatrists, promoting discussions and increasing our understanding of our ethical responsibilities.

We want to point out that we have not in any way indicated that we have the answers

to these moot questions in psychiatric ethics. We only point out their existence. We also hope that additional discussion between ourselves and our colleagues in the law profession might ensue. Any attempt to further our understanding of our ethical responsibilities can help us, for as Hippocrates stated, if his oath is adhered to by doctors, then "may I enjoy honor in my life, and art among all men for all time: but if I transgress and forswear myself, may the opposite befall me." And, as Galdston(6) has written, "It is not men who ennoble medicine, but medicine that ennobles men."

If this paper has in any way brought about a wish for individual reflection on this subject, and for further discussion and clarification, we shall consider that it has been worthwhile.

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## CLINICAL NOTES

### THE SAFER ADMINISTRATION OF SUCCINYLCHOLINE WITHOUT BARBITURATES—A NEW TECHNIC

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The advent of the tranquillizers has not displaced electroshock therapy (EST) as the most effective treatment for certain mental disorders. Since EST will be used until a better method of treatment is found, it is essential that it be made as safe as possible.

The recently introduced muscle relaxant, Succinylcholine (SCC), by practically preventing all fractures, has made EST much safer. By reducing the work load on the heart during a treatment, it also has made it possible to treat poor-risk cardiac patients, many of whom would otherwise have been denied the beneficial effects of EST. However, SCC in the doses commonly used (20 or more mg.) is not without danger, as it not infrequently causes apnea. Some deaths have occurred with its use.

Because of these reactions, many psychiatrists using EST have not availed themselves of SCC at all; and a number of others have administered it only with the aid of an anesthetist. In the latter instance, the cost of the treatment to the patient has been increased.

To overcome these disadvantages, I have developed the following technic which I have already successfully used in about 100 patients:

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The selection and preparation of the patients are exactly the same as for the usual technic. The patient is given 1/50 of a grain of Atropine intramuscularly, one-half hour prior to treatment. No barbiturates are used. Just prior to the treatment the patient is quickly given 10 mg. or less of SCC intravenously. Ten seconds later, a petit mal EST stimulation is given and this is followed 20 seconds later with the usual grand mal EST stimulation. From here on, the patient is handled in the usual manner.

This procedure produces adequate muscular relaxation with practically no apnea. This statement will be almost impossible to believe without witnessing a treatment. Nevertheless, using 10 mg. of SCC practically all muscular relaxations produced are of the order of 3 or 4 plus; and what is more important many of the patients breathe spontaneously at the end of the fit and do not require oxygen or artificial respiration.

It does not give rise to anxiety as the petit mal stimulation effectively obliterates the memory for any disagreeable sensations the SCC might have caused. It makes EST much safer and it makes it possible to administer the treatment without the aid of an anesthetist.

I believe that these advantages are so beneficial that the profession should be apprised of this technic as soon as possible.

## CASE REPORTS

### GASTRO-INTESTINAL HEMORRHAGE AS A COMPLICATION OF RESERPINE ADMINISTRATION: REPORT OF TWO CASES

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Among the recognized pharmacological actions of the Rauwolfia compounds is the stimulation of secretion of gastric acid (1, 2, 3). To date, however, there have been very few reports of clinically observed gastro-intestinal complications of serious degree following the use of one of the Rauwolfia group (4). It is therefore considered worth while to report the following 2 cases:

H. P., a 36-year-old female, had been hospitalized at the Agnews State Hospital almost continuously since 1943, with a diagnosis of schizophrenic reaction, paranoid type. She had received intensive somatic therapy previously as well as individual psychotherapy. A bilateral lobotomy procedure was carried out in 1948, with very little subsequent change in her behavior. She was started on reserpine September 17, 1954, at which time she was described as hallucinatory, manneristic, grimacing, noisy, profane, restless, and asocial. At no time had she any gastro-intestinal complaints or observable signs of gastro-intestinal dysfunction. Initial dose of reserpine was 1 mg. a day orally, which was increased on November 17 to 3 mg. daily, as she continued overactive and restless. On November 30 the dosage was increased to 4 mg. daily and maintained at this level. On June 6, 1955, the patient fainted early in the morning and shortly afterward passed a semi-liquid black stool. Blood pressure was 92/50, pulse 66 and regular. There were no abdominal complaints elicited from the patient and no nausea or vomiting observed. Hemoglobin was 7.4 gm., hematocrit 22%, sedimentation rate 36 mm. per hr., WBC 8,600. A phenolphthalein test for occult blood in the stool was strongly positive. Bleeding time was 1 min., coagulation time 3 min., 30 sec. A barium meal was carried out June 21, 1955, and the radiologist reported the presence of a central duodenal ulcer. The patient responded to conservative treatment and has had no further symptoms to date. The reserpine has not been resumed.

R. C., a 41-year-old female had been mentally ill since 1945 and periodically hospitalized since that time. Extensive therapy included a lobotomy procedure done in 1954 at the Langley Porter Clinic in San Francisco. Subsequently periods of catatonia

or combative, quarrelsome behavior continued. She was started on reserpine September 13, 1955, with an initial dosage level of 3 mg. per day by mouth. Additional electroshock therapy was required. The reserpine was discontinued for a period of several days in December and then resumed, and on January 3, 1956, the patient complained of pain in the left chest anteriorly and scapular area. There was a very slight temperature elevation and frequent belching. On January 4, 1956, hemoglobin was 7.7 gm., hematocrit 23%, sedimentation rate 47 mm. per hr. A phenolphthalein test for occult blood in the stool was reported as 4+. An electrocardiogram was essentially normal. Fluoroscopic examination following barium meal was attempted on January 16 but was not satisfactory because of inadequate patient cooperation. No definite abnormality was noted at this time. Further studies including bleeding, clotting, prothrombin time, and platelet count were within normal limits, as were the thymol turbidity and cephalin flocculation tests. On January 31, the phenolphthalein test for occult blood was still strongly positive and a repeat upper G.I. series demonstrated a hiatal hernia but no ulcer was found. On withdrawal of reserpine and conservative treatment evidence of bleeding from the gastro-intestinal tract subsided and the patient's general condition improved. There have been no further similar symptoms to date.

#### SUMMARY

Two cases of bleeding from the gastro-intestinal tract during the course of reserpine administration in the treatment of mental illness are described. The fact that both patients had been under hospital observation for several years without previous evidence of gastro-intestinal disorder lends credence to the presumption that the reserpine was implicated as an etiological factor. It seems particularly noteworthy that one patient did not complain of any discomfort until bleeding had assumed serious proportions and the other patient never did complain of illness. As many patients are undoubtedly under treatment with reserpine who are suffering

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from the type of mental illness which does not permit ready communication to the nursing personnel of signs and symptoms of physical illness, the physician must be alert to the possibility of major complications without dependence upon the patient's complaints. The fact that both patients had undergone a lobotomy procedure is of interest but is considered probably coincidental.

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### PSEUDO-TABES SYNDROME AS COMPLICATION OF TRANQUILIZER DRUG THERAPY

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Practically every psychiatrist has seen remarkable improvement in certain patients administered tranquilizer medication. But in rare instances complications are very serious, particularly those of a neurological category. Therefore it is essential to be alerted to the various types of complication that may be encountered.

The Parkinson-like syndrome is the most frequent neurological complication seen, and this practically always disappears in a short time if the tranquilizer medication (whether reserpine or chlorpromazine) is discontinued.

This report concerns a type of neurological complication caused by modest dosage of Thorazine preceded by reserpine, that has not been mentioned in the literature heretofore. A 47-year-old man, referred for neurological opinion, presented a typical clinical picture of tabes dorsalis. There was such extreme ataxia that he could not walk unaided, and in the Romberg test he would fall unless supported by some other person. He had fallen several times sustaining bruises. In walking, even when guided by someone, he staggered about like one thoroughly intoxicated and again, he would fall if attempting to walk unaided. Typical Argyll-Robertson pupils were found and there was a flail type of muscular weakness in the neck, so marked that the head would flop over backward intermittently, quite involuntarily.

There was decreased vibratory sensation extending up to both knees, and knee reflexes were absent bilaterally (even with reinforcement). Speech garbling was quite marked in test phrases, and organic mental changes

were present, particularly memory defects. There was a noteworthy dark grey coloration of facial skin.

Because tabes of specific origin is so rarely seen today it was believed to be due to the medication he was taking. However, in order to rule out cerebrospinal infection immediate lumbar tap was carried out, and the serology was completely negative for specific disease. But it was difficult to believe that the extreme flail foot-drop steppage gait, not described heretofore could be of drug causation. Laboratory work disclosed no evidence of agranulocytosis—there was instead a moderate leucocytosis (R.B.C.: 4,810,000; W.B.C.: 15,200. Differential: polys 78%; lymphos 20%; monos 2%). The leucocytosis decreased over the next 4 weeks to 12,200 with differential: polys 71%; lymphos 25%; monos 2%; and metamyelocytes 2%.

The history follows: The patient had been subject to severe nervous disorder in the past. He had been administered a course of electroshock therapy 5 years before (by the writer) for a melancholy type of breakdown, with full recovery. Also, having been subject to severe gastric symptomatology, his surgeon had performed a gastroenterostomy some years ago. With a recurrence of nervous symptoms 7 weeks ago his physician prescribed reserpine 0.25 mg. orally t.i.d., and when he was no better in a week, the medication was changed to Thorazine 25 mg. orally t.i.d., which he had taken regularly for the next 5 weeks, when ambulatory disability occurred.

Strangely his family had not noted the neurological disorder till fully developed and he was unable to walk.

Proof of the drug causation lies in his complete recovery from the disabling syndrome in 10 days with no medication of any kind. Only discontinuance of Thorazine was necessary. It was planned to place him on Cogentin therapy if he had not improved by withdrawal of the tranquilizing drug, but improvement was so rapid that this was unnecessary.

Because tranquilizers are being prescribed

so widely today and are being increasingly used without prescription (enhanced by the wide publicity these "wonder drugs" receive in the metropolitan newspapers), other neuropsychiatrists will doubtless encounter similar neurological complications.

At this time, when the fictional Utopia induced by the generalized use of euphoria-inducing "soma," described in Aldous Huxley's *Brave New World*, is once more being popularized, it seems necessary for all doctors to be forewarned of all possible complications that may occur.

#### BENJAMIN RUSH

Benjamin Rush, in his 31st year, was the youngest of the Signers from Pennsylvania and one of the very youngest men in Congress. . . . He graduated from the College of New Jersey (Princeton), began the study of medicine in Philadelphia, completed it in Edinburgh and London, and became the most famous American physician and medical teacher of his generation. . . . He was associated throughout most of his professional life with the Pennsylvania Hospital and the University of Pennsylvania, and he was a leading light of the American Philosophical Society. Like Franklin and Jefferson, he was interested in everything, and he passed his days "like an arrow shot from a bow. . . ."

He was the hero of the yellow fever epidemic of 1793 in Philadelphia, though William Cobbett criticized—not without reason—his practice of blood-letting. Rush never wholly convinced his scientific friend Jefferson that the doctors of that time did more good than ill. . . .

It was to Rush that Jefferson wrote privately, in the election year 1800, "I have sworn upon the altar of God eternal hostility against every form of tyranny over the mind of man"; and, toward the end of his own life, the Doctor, as the self-appointed intermediary between Jefferson and Adams, brought about the renewal of their correspondence. . . .

He was at the height of his fame in 1813 when he died in Philadelphia in his 68th year.

His comment on himself consists of three words: "He aimed well."

—DUMAS MALONE  
(*The Story of the Declaration of Independence*)

## HISTORICAL NOTES

### AN EARLY NEURO-DRUG EXPERIMENTER: ALEXANDER MONRO II (1733-1817)

Modern neuro-drug experimenters will be interested to learn about one of their ancestors who, in an amazingly modern way, almost 200 years ago approached the problem of the influence of drugs upon the nervous system. This experimenter was Alexander Monro II, the astonishing, polyphonically inclined "professor of surgery" at Edinburgh. Medical historians have already credited him with several important, original discoveries about the lymphatic system and anatomic structures. His name is linked with the discovery of the "foramen of Monro."

History, however, has almost completely ignored Monro's experiments on the influence of drugs upon the nervous system, which he attempted by applying opium injections to various animals. Monro first reported these attempts in 1771, but they seem to have occupied him fairly continuously for about 25 years. In a publication entitled "Experiments on the Nervous System with Opium and Metalline Substances" (Edinburgh, 1793), Monro surveys his own attempts so impressively that we can sum them up best by quoting his own major sentences:

I cut one hole in the fore and upper part of the cranium and dura mater of a frog and another in the back part of the lower-most vertebrae, and then injected, from the one hole to the other, a small syringe full of water, in five ounces of which one ounce of opium had been infused for three days. The infusion, by this means brought into contact with the whole surface of the encephalon and spinal marrow, produced almost instantly universal convulsions; and, in less than two minutes thereafter, the animal was incapable of moving its body from the place where it was laid. A quarter of an hour thereafter, I found the heart beating twenty-

five times only in the minute; and so feebly, that it could not entirely expel the blood. When half an hour thereafter, the sciatic nerves were pinched, a slight tremor only was excited in the muscles of the leg; and animal electricity produced but feeble twitchings of the muscles.

The infusion of opium injected in the same manner in rabbits and in a pig, produced similar effects.

Continuing this report, Monro discusses the theories of Felice Fontana, whose book on the effect of poisons was receiving much attention at that time. In his book Fontana maintained that poisoning occurs by way of the blood-stream. Monro doubted the correctness of this thesis, and after reporting a set of experiments disproving that the toxic influence is carried primarily by the vascular system, he presents his case in the following climaxing statements:

Many years ago, I found, after cutting the venae cavae and aorta of a frog, that a watery solution of opium poured into the heart, occasioned, in a few minutes, convulsions in its legs; and, after cutting out the heart, that the opium poured into the cavity of the abdomen affected the legs in like manner; although, in these experiments, the circulation was not only interrupted, but the greater part of the blood evacuated. I therefore then concluded, and now conclude, that opium and other poisons, even after they are mixed with the mass of blood, produced their fatal effects, chiefly and almost solely, by acting on the nerves of the heart and vascular system, and through these, affecting the whole of the nervous system.

In connection with the active experimental work of today in neurophysiology it seems worthwhile to recall Monro's experiments of 200 years ago.

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## CORRESPONDENCE

### PSYCHOANALYSIS

*Editor, THE AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: Reading the recent article by Hiram Johnson, M. D., entitled "Psychoanalysis, Some Critical Comments," in the July 1956 issue of *The Journal* has made us feel very strongly that some reply is required. This article is only one among several which have appeared in *The American Journal of Psychiatry* in recent years which we feel has given a very distorted picture of psychoanalysis not at all in keeping with analysis as it is actually taught and practiced in America. We, incidentally, feel ourselves to be "orthodox psychoanalysts" and it is "orthodox psychoanalysis" that is specifically criticized.

The appearance in print of such articles, it seems to us, raises two very serious questions. First of all, what does it imply in regard to the editorial policy of the *Journal* that articles so obviously full of misinformation are published, and, secondly, we feel that certain statements and interpretations in the body of the paper need to be specifically answered and clarified. We shall try to mention only a few of the more important.

Early in the article the author refers to the "polarity between the male and female principle" and goes on to state, "Frankly, I find this unintelligible." Later in the same paragraph he refers to it as "pure poetry." We do not criticize the lack of understanding, but we do feel that this should not be the basis for the repudiation of ideas. The principle of bisexuality is certainly one that is generally accepted, not only by analysis but by psychiatrists in general, and has been clinically demonstrated time and time again. Also it is a basic tenet for the understanding of human behavior.

Let us turn to another contention. The author refers to a "slight flavor of misanthropy" in what he calls pathographic literature. For example, he states, "nowhere

do I recall ever having seen psychoanalytic efforts expended to prove that "X" was not a homosexual." Thus it is clear that the author considers the observation that "X is a homosexual" an accusation. He confuses scientific observation with judgment statements. We can cite another instance in which this confusion is also very clear: "Pathography is not the emotionally detached scientific instrument that it purports to be, but is too frequently a distillate of bitterness containing in its philosophic essence a denigration of man."

The next major section of the paper is devoted to the idea of "negative conditioning" or "reciprocal inhibition." This entire section shows a complete lack of awareness of analytic procedure. Certainly it is true that undesirable characteristics and attitudes do come to the patient's attention. Such awareness is necessary, as is also the awareness of his more positive qualities, but this is not the essence of the curative process but is simply a necessary by-product. In fact, it is necessary for any patient to overcome feelings of "horror and disgust," as well as anxiety, in order to permit full access to the necessary insight which comes to light during the process. It is only then that these previously pathogenic infantile strivings can be integrated and usefully sublimated.

The final section of the paper implies two things: first, that Freud from the beginning and psychoanalysis currently subscribe to the thesis that all of man's difficulties stem from "psychosexual anxiety," and, secondly, the author implies in present-day life all problems arise from "existential sources," that is, the problems of existence and of reality. We would simply like to point out that neither Freud nor psychoanalysis since Freud has ever held such a narrow view. Furthermore, it would seem in making such an interpretation of analytic thinking the author is unaware of all the work in ego psychology which has occupied the attention of analysts for the past 30 years. We would

also like to voice our opinion that going to the opposite extreme of attributing all man's difficulties to external factors would be a concept as fallacious as the one criticized.

Finally, we would like to return to our first question. For the most part we find no occasion to criticize the material which appears in the Journal. We may not find ourselves in agreement but do regard the presentations as unprejudiced and tending to encourage further study. However, from time to time papers such as the present one do come into print and it seems to us that they represent a divergence from the usual constructive editorial policy. We would like, therefore, to urge an editorial attitude toward papers concerning analysis which is as critical and discriminating as that toward the other types of material which appear in the Journal.

We appreciate this opportunity for expressing these views and we hope that this note may contribute to the clarification of these most complex problems.

JAMES F. BING, M. D.,  
FRANCIS McLAUGHLIN, M. D.,  
Baltimore, Md.

*Editor, THE AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: I read with considerable interest the paper entitled, "Psychoanalysis: Some Critical Comments," by Hiram Johnson which appeared in the July 1956 issue of the Journal.

As one who has had more than a nodding acquaintance with psychoanalysis and psychoanalysts for many years, I found Dr. Johnson's incisive and scathing criticisms both stimulating and somewhat disturbing. I think that much of what he says has considerable validity. For example, his suggestion that much psychoanalytic writing is "pure gibberish" will find support in many

quarters including some intelligent and competent members of the psychoanalytic fraternity. I think his attack on the psychoanalytic party-line also merits thoughtful consideration as do his indications regarding the lack of scientific validation of psychoanalytic principles and theories. Some of my psychoanalytic friends, who are good physicians first and psychoanalysts second, I am sure have similar concerns.

However, if the current resurgence of attacks on psychoanalysis, of which Johnson's is the latest to come to my attention, continue without the appropriate restraints of objectivity, might this not lead to the dangerous phenomena of sweeping condemnation and intolerance? Without appearing to be presumptuous, I would like to suggest that there is much in the work of Freud and later analysts which has considerable merit. What is desperately needed in this troubled world is the painstaking process of unbiased evaluation and criticism supported by rigorous research (and I don't mean much of the nonsense that is labeled "research" which appears in the literature) rather than blanket disapproval of any particular theories and procedures. If I may be permitted to use a rather old cliché, I hope that the baby is not thrown out with the bath water.

In closing I would like to add that I wish Dr. Johnson would bestow his literary talents to the field of social work which for the past three decades probably has done (and is still doing) more than any organized profession to promote Freudian psychology in the United States. If some of his critical views were published in a social work journal I feel that the shock treatment might be highly therapeutic for some social workers and I hope not too traumatic for others.

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#### REPLY TO THE FOREGOING

*Editor, THE AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: First of all I shall take up the letter of Dr. Bing and Dr. McLaughlin point by point.

"Early in the article the author refers to the 'polarity between the male and female principle' . . . and 'he refers to it as pure poetry.'" Let me affirm with some asseveration that I am fully aware of the fact that

there are two sexes. I simply wish to criticize such language as "male and female (yin-yang) principle." For in this context exactly what does "principle" mean? This calls for clarification if it is to mean anything. Otherwise we land in the realm of Platonic ideas where the "male and female principles" would rank with the Good, the Beautiful, Justice and the rest of Plato's ethereal archetypes. If the writer means this, he should say so or use clearer language.

"The next major section of the paper is devoted to the idea of 'negative conditioning' or 'reciprocal inhibition.' This entire section shows a complete lack of awareness of analytical procedure." In answering this, I must affirm that I have a most complete grasp and awareness of analytic procedure. For I underwent an analysis (orthodox) for over 10 months with the mounting conviction that the procedure was not scientific. For it soon became evident to me that the keystone of the process, the "free associations," was bogus. Let me emphasize that this is the bedrock of my argument which stands or falls with this thesis. I was soon made to understand by means of questions put to me, by means of "suggestion," emotional attitudes, eloquent silences and other cues, that certain of my productions were useless (resistance) and that other productions were good (i.e. had negative conditioning impact). ["He was quickly through with his associations; so the analyst tried to help him out. She suggested that perhaps. . . ." (From French's *Integration of Behavior*, p. 81).] An analysis conducted by correspondence like an international chess game (or otherwise where cues were carefully screened out) would never work. This is because psychoanalysis is an active hypno-suggestive process, although obliquely so (dreams are important—they differ for each type of analysis). It is a technique which requires training, psychological cunning, and the ability to handle an elaborate apparatus of cues. Negative conditioning is the essence of the process. This negative conditioning (largely through bogus "free" associations) is used by the analyst as hammer and chisel are used by the sculptor—for the purpose of personality molding. More specifically, for the purpose of personality molding in the

direction of financial and psychosexual success, not as by-products, but as ultimate goods. And this is a brand of hedonism technically called Cyrenaicism, a philosophy which this writer rejects.

"The author implies in present-day life all problems arise from 'existential sources,' the problems of existence and reality . . . the opposite extreme of attributing all men's difficulties to external factors would be a concept as fallacious as the one criticized." First of all, this writer did not use, or certainly never meant to use, the word "all." In scientific polemic, the word "all" belongs only to formal logic and mathematics. Moreover, those conversant with existential writing will know that existential thinking does not attribute all men's difficulties to external factors but precisely the opposite. My critics' definition of existential anxiety is quite puzzling. I have tried to use the concept of existential anxiety as it is used by Heidegger and Tillich and in conformity with present-day analytic practice.

In thinking over the letter from Dr. Bing and Dr. McLaughlin, it seemed to me that it was much more eloquent in its omissions than in its affirmations. There is no reference, for example, to the philosophical ingredients which I have pointed out as implicit in psychoanalysis. This I consider to be by far the most important feature of my slight contribution. In another part of my article (p. 40) after careful deliberation, I pointedly raised a question which I will quote in full: "Is it possible that we are developing the equivalent of a secular church, supported by government monies, staffed by a genital-level apostolate unwittingly dispensing a broth of existential atheism, hedonism, and other dubious religio-philosophical ingredients?" If my colleagues were sufficiently endowed with, let us say, the yang principle, it would seem to me that they would want to say something about this; for the language above is either immoderate—or true. Nor is there any reference to my statistics of suicide among psychoanalysts (although a miniature face-saving hagiology is arising around many of the defunct, more wonderful than Voragine). But enough of this unpleasant topic. This is the inevitable by-product of a religion not true for Man.

Certain aspersions contained in this letter on the editorial policy of the Journal seem to me, to say the least, astonishing. The Journal traditionally has maintained a high scientific standard. This calls for a periodic scrutiny of the fundamental theory and premises underlying all psychiatric procedures and disciplines—barring none. Attempts to browbeat the editorial policy of the *American Journal of Physics* for giving space to papers criticizing the quantum theory would be unthinkable. This is because the *American Journal of Physics* specializes (in a certain area) in general theory and fundamental premises. Actually what my esteemed colleagues most resent in my article is a frank examination of fundamental psychoanalytic theory. This they resent with the animus of religionists whose articles of faith are being weighed. And this is true of alchemy, astrology, palmistry and all cults. One must never, never, examine the fundamental premises. Unfortunately, this is the cardinal concern of science. It is all as simple as that.

My answer to Dr. Andriola's temperate and intelligent comments will be brief. Although the process of beatification has been suspended in the Anglican Communion since 1523, there was a testimonial dinner in honor of Sigmund Freud's Centennial on May 6 last at St. John's Cathedral in New York City. This was arranged presumably by psychoanalyzed priests in connection with the Cathedral Psychoanalytic Clinic. Thus the largest cathedral in the Western Hemisphere pays homage to the most influential atheist of the twentieth century. This instance is given to show which way the wind blows. The point is that there is little danger of throwing the baby out with the bath water. The baby has grown too big. Not only has psychoanalysis infiltrated religion (especially amongst its nonviable forms), but also the medical schools, universities, and, as Dr. Andriola points out, the social service schools. And here Dr. Andriola suggests that I write some salutary articles for social work journals. This would accomplish nothing. For it is my general impression that most social workers believe what they are told to believe. If by some cosmic freak the

school faculties were dynamically reoriented overnight to Zend Buddhism, there would only be the mildest sussurus of protest in the classrooms. Much more serious is the problem in some medical schools where the students are not in the possession of clinical facts in order to make judgments and where many sound men turn away to other specialties. Also a few resident training hospitals—"dynamically oriented," to be sure—offer the bizarre picture of a parasitic mental hygiene clinic set up within the framework of the hospital for the benefit of the residents (usually there is trouble getting suitable office-practice type patients for teaching—psychoneurotics or bogus "schizophrenics") whilst the preponderant bulk of the psychotics are handled by senior physicians and generally ignored teaching-wise. (Even Jaspers has not yet been translated whilst Storch has been out of print for 5 years and will not be reprinted). Of even more concern is the situation in some departments of clinical psychology where artificial nosological entities are taught (bolstered up by a crazy scaffolding of Rorschach halo-effects), where both teacher and student have no contact with state hospital wards and where rat-statistical disciplines have supplanted the liberal arts education of a former generation. Worst of all, the clinical psychologist has no Hippocratic Oath.

But all things have cycles and there are several factors which now threaten the arrogant overlordship of the fatalistico-athistico-mechanistico-deterministico-hedonists (to spell the thing in full), forcing psychoanalysis into a more reasonable, eclectic approach. The first factor will take time to make itself felt. It is a growing tendency in medical schools to re-emphasize the humanities in premedical education. For there is no better antidote for myopic thinking than a sound understanding. The second factor working for eclecticism will have a more immediate effect. It is chemotherapy. Already as I write this, the chill wind of the ataraxic agents is blowing across the analytic couch.

HIRAM K. JOHNSON, M. D.,  
Rockland State Hospital,  
Orangeburg, N. Y.

## FRENQUEL

*Editor, THE AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: One of us (W.J.T.) has reported in this Journal (Sept. 1956) on the failure of Frenquel to influence the psychosis in chronic, institutionalized, schizophrenic women. The question was raised by another (R.K.) as to the adequacy of this evaluation. Accordingly, 30 schizophrenic male patients were selected on a disturbed ward of this hospital (Central Islip State Hospital), each having had a history of auditory hallucinations. Some had had prefrontal lobotomy and some were complicated by mental deficiency. The patients were first rated, using the Lorr Multidimensional Rating Scale.

Frenquel was supplied to us by Dr. Raymond C. Pogge of the Wm. S. Merrell Company. Medication was started by intravenous injection of 15 mg. This was followed by administration of 20 mg. by mouth 3 times a day for 30 days; 3 patients refused medication; 2 were not hallucinating when first interviewed. At the end of 30 days all patients were re-rated and the impressions of the personnel most familiar with the patients were considered. Where a difference appeared between the premedication and the postmedication ratings, the data were subjected to statistical analysis.

The only item which showed a significant change was that pertaining to visual hallucinations. However, in a discussion by the group which included all raters and ward personnel, it became clear that this "significant change" related, not to the patients, but to the variations in inference on the part of the raters. These variations we have been taught to expect in the use of rating scales in work of this sort. In only one case did there appear to be any alteration in a patient during this test, in that he became more sociable, more interested in recreation, more cooperative, and no longer responding to hallucinatory voices. The data available, however, suggest that this could be attributed as well to the 3-times-a-day contact with the personnel as much as to the Frenquel itself.

In conclusion, we find no support for the assertion that Frenquel exerts any action on schizophrenics or on those who suffer with hallucinations.

ROMAN KUZIW, M. D.,  
MARK JACOBY, M. D.,  
LAWRENCE GROLNICK, B. S.,  
W. M. J. TURNER, M. D.,  
Central Islip State Hospital,  
Central Islip, N.Y.

## PSYCHIATRY IN GUATEMALA

*Editor, AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: Reference is made to the letter of Dr. Bennett from Berkeley, California, published in the June issue of the Journal, under the heading "Psychiatry in the Caribbean Area." Aside from Dr. Bennett's personal impressions and recommendations which I certainly do respect and share, my intention is to let it be known to the readers some of the other aspects of the work that is being done in the field of psychiatry in Guatemala; aspects which he unfortunately missed, having only seen one single institution in the country, that which precisely is not carrying out its program of work at the present time. It is very true that working conditions have been rather difficult in Guatemala for a long time. But since 1954, closely related to the

latest political revolution, a revolution in psychiatry as well as in medicine has been on the way.

This refers to a wide range of activities leading to an over-all approach to the health problems in terms of national necessity. The government is most interested in pointing out the needs of the people in order to fight disease at large. This, of course, will take some time to develop from research projects into action. Some progress has been made, however, in a short period in the field of psychiatry. A National League of Mental Hygiene has been created and set to work on this very problem. Interest has increased among teachers, social workers, lawyers, pediatricians, and general physicians. A mental hygiene program will soon reach the schools and

other social areas—this as far as preventive psychiatry is concerned. On the other hand, 2 outpatient clinics were opened this year, to carry on treatment and diagnosis for children and adults.

Another clinic is in charge of the problems of the elementary school population. Two of the major state institutions can afford now to have a well-developed department of psychology and psychiatry. As for the educational aspects, more stress is being placed on the teaching of applied and dynamic psychology, psychopathology, and psychiatry in the schools of medicine, psychology, social work, nursing education, etc.

Treatment facilities for the mentally ill are still highly deficient in institutions, but a recent meeting on public hospitals and institutions called forth by the Department of Public Health has stressed the need to change and improve our substandard methods and old-fashioned institutions.

Certainly we face now a tremendous shortage of trained personnel, which I consider

far more important than the eternal riddle of financial support.

What we need is trained people to work and work hard. The psychiatrists in Guatemala barely number one-half dozen, for a population of 3 million, and they have to do most of the leading work in the field. Three of them are A.P.A. members, and one, the writer, was trained and lived in the U. S. for 7 years.

We would have enjoyed showing Dr. Bennett our various institutions and the activities we are engaged in and to have profited by his comments and suggestions. We would have liked to have him address our professionals and students in a public speech. Finally, we do entirely approve of Dr. Bennett's comment that we need to be helped in our problems by the Association—which he claims should be a Psychiatric Association of the Americas.

AUGUSTO AGUILERA, M. D.,  
Guatemala City, Guatemala.

#### REPLY TO THE FOREGOING

*Editor, AMERICAN JOURNAL OF PSYCHIATRY:*

SIR: My recent letter, "Psychiatry in the Caribbean Area," necessarily concerned only those institutions and persons that I could visit in the time available. I wrote my comments mainly to encourage the psychiatrists and other workers in their valuable work. I, of course, agree with Dr. Aguilera that the greatest need is for trained personnel. On

another trip I hope to be able to see more institutions and to note improvements. I am also pleased that Dr. Aguilera feels the need for help from our Association, and approves of the idea of one psychiatric association for the Americas.

A. E. BENNETT, M. D.,  
Berkeley, Calif.

#### EDUCATION

Nothing should be taught in any school that the teacher does not know. Beliefs, superstitions, theories, should not be treated like demonstrated facts. The child should be taught to investigate, not to believe. Too much doubt is better than too much credulity. So, children should be taught that it is their duty to think for themselves, to understand, and, if possible, to know. Real education is the hope of the future.

—ROBERT G. INGERSOLL (1833-1899)

## PRESIDENT'S PAGE

The news that the old Pennsylvania Hospital for Nervous and Mental Disease at 44th and Market Streets in Philadelphia has been sold to the city government and that the buildings are eventually to be razed is sad news indeed. Though the hospital will rise anew, modernized and resplendent, on the 49th Street site, still withal, this particular segment of this venerable institution is inextricably entwined with the history of what is now The American Psychiatric Association. It was within its confines that a large segment of the history of American psychiatry was written. In more modern times it mothered many men who head various psychiatric departments and institutions throughout the land.

Coming as it does upon the heels of Butler's closing—another institution replete with historical significance—the change in façade of this institution heralds, if not the end, at least the beginning of the end of an era. It was these old private hospitals which for the most part kept psychiatry alive for almost a century, but their role has changed now and time, with its inexorable demands, requires that they too must change.

Though grim, remote and forbidding in external appearance, these institutions had a quiet dignity within. They were usually directed by dedicated men who withstood numerous onslaughts and made up in humanitarian devotion and attention for what they and the age lacked in scientific skill. Not at all strange to say, their recovery rates compared rather favorably with those of our present day.

Now, as research institutions arise and general hospitals add psychiatric wards and pavilions and as our state hospitals give promise of beginning an upward climb, we are in a fair way to forget these old hospitals in question and to regard them with the polite askance which is usually reserved for the Kraepelinian era. They were not built to operate in an economy such as our present one and they were not built to operate in the hustle and bustle of a populace in a hurry. Whether the changes which are now required of them are for the better only time will tell, but one thing is certain: the unselfish spirit

and devotion which usually animated these institutions and carried them through bleak years must be preserved.

Without intending to be in any way funereal, lugubrious or reactionary, these memories do point up a serious problem which faces us today. Our present day hospitals, Federal bureaus, military, veterans and state institutions are all desperate in their need for men to man them, competent men who will carry on the humanitarian work for which they were instituted. In all too many instances now tasks which traditionally were done by physicians have passed to representatives of other disciplines by default. It is not unusual to see important psychiatric posts in federal or state governments go begging for lack of interest by competent psychiatrists. The danger here, of course, is that government officials will tire of trying and importuning young physicians to take upon themselves these tasks and will rewrite the specifications in a manner not satisfactory to anyone concerned. This has already happened in several instances and it bids fair to happen again on even a wider scale.

Granted, the trouble is not all on the part of the doctor and at times selfish interest and partisan parties have discouraged the young physician and driven from him all thought of carrying out official tasks. Unfortunately, it is the patient who suffers because of this discouragement and unfortunately, despite our obvious advances, there are still three-quarters of a million of these patients in our mental hospitals.

There is a desperate need for men to enter the public service. Difficult as it is and intangible as are its rewards, they are there nonetheless and in lasting fashion. How this feat of filling the depleted ranks of our institutions is to be accomplished is for wiser heads than mine to solve, but solved it must be if we are to carry out the humanitarian traditions which the men who manned those old hospitals left us as a heritage.

A poor man served by thee shall make thee rich;  
A sick man helped by thee shall make thee strong;  
Thou shalt be served thyself by every sense  
Of service which thou renderest.

FRANCIS J. BRACELAND, M. D.

## COMMENT

### THE ACADEMIC LECTURE

The thoroughgoing attack on modern psychiatric treatment methods by the Academic Lecturer at the Annual Meeting of The American Psychiatric Association in Chicago recalls vividly a no less vigorous assault upon American psychiatry just 62 years earlier. The occasion was the semi-centennial of the founding of the Association; the place, Philadelphia; the lecturer, Weir Mitchell. It is safe to say that none of those who listened to Dr. Bailey could compare notes by reason of having also heard the punishing words of Dr. Mitchell in 1894. Presumably however many will have read his address; and to those who have not, it is warmly recommended. It will be found in the separately published Proceedings of the American Medico-Psychological Association (as The American Psychiatric Association was then called), vol. 50, pp. 101-121.

In opening his lecture Dr. Mitchell explained that when invited to be the speaker "on this important anniversary" he had declined. "It is customary on birthdays to say only pleasant things, and this I knew I could not altogether do." When still pressed to speak he reflected "that men who could thus ask the criticism, which they knew must come without mercy, were well worth talking to." And he *did* speak. He lashed out against the whole system of mental hospital management, against that "inconceivably shameful thing" political control and meddling. But it was the hospital superintendents themselves, the men sitting before him, to whom most of his criticisms were addressed. Speaking for the profession at large Dr. Mitchell said, "We do not believe that you are so working these hospitals as to keep treatment or scientific product on the front line of medical advance." In painful detail he enumerated the shortcomings of the mental hospital procedures of that day. He pilloried prevailing "hospital torpor."

There were of course hurt feelings and resentful reactions. Hospital life was not everywhere sluggish routine. There were

those who were striving for better things. The stirrings of reform were in the air. But credit must be given to the bombshell Weir Mitchell dropped in the asylum camp for more drastic improvement in institutional practice and an accelerated pace to catch up with general medicine.

The contrast between the Mitchell and the Bailey addresses is striking. Mitchell was lecturing the asylum doctors; Bailey was devaluating current treatment procedures. Mitchell criticized the neglect of treatment measures that were available; Bailey the too zealous pursuit of therapeutic experiments and theories. Mitchell's target was inertia; Bailey's overactivity—on the one hand, the *laissez faire* of custodial care; on the other the promulgating of panaceas.

There have been criticisms of Dr. Bailey's pronouncements just as there were of Dr. Mitchell's in 1894. Neither of these critics had long-term first-hand experience as a physician in mental hospitals, in constant contact with the enormous problems such practice presents, and lacking withal sufficient funds and staff.

Many of the deficiencies that Weir Mitchell complained of have been made good. The features of the ideal hospital of the future that he described have become realities. His prophecy that "the years will surely bring something like, or far better, than what I have sketched" has been widely verified. The scientific spirit pervades the mental hospitals as never before. The question Dr. Bailey raises is whether this spirit is always tempered by balanced judgment based on sound training, ample experience and historic perspective. That there has been too much or premature optimism in many quarters in the application of newer remedies, whether physiological, pharmacological or psychological, probably no one would deny. That does not in the least mean that such therapies should be condemned out of hand.

Are Bailey's strictures too sweeping? There will be protests that they are. His

sharpest barbs he reserves for Freud and psychoanalysis to which he devotes the bulk of his polemic.<sup>1</sup> In recent years probably no one of like eminence has gone all out in such fashion. The main significance however of Dr. Bailey's criticism is the indication that the sixty-year old Freudian discipline is still a controversial issue—lively disbelief over against assured acceptance and devotion. Here as in the field of religion there will be the Theresians, the Laodiceans and the Galionians of the Oslerian classification.

Beyond question psychoanalysis has gained wide ascendancy in the United States, especially among the younger generation. Freud's greatest living disciple, on his recent visit to the United States, referring to the state of psychoanalysis on this side of the water, is quoted as saying, "There's more work being done in America than in any other country—or in all other countries combined." It is easily credible.

But Dr. Bailey, although he gave his main attention to psychoanalysis, did not spare the newer treatments—lobotomy, insulin, electroshock, the tranquilizing drugs; all received their full measure of castigation. But he did make concessions. He does not deny the usefulness of electroshock or the newer pharmacotherapy currently on the upsurge. He states clearly that the ataractic drugs may calm an agitated patient and render him more accessible to other forms of

therapy. And he finds that electroshock, in involutional depression "sometimes cuts short the attack in spectacular fashion." Surely this is not therapeutic nihilism. One wonders whether, having seen the same thing happen over and over again in the tortuous history of psychiatry (and Bailey deprecates the lack of historic perspective in so many psychiatrists of today), he is not deliberately couching his criticisms in such vigorous, even exaggerated language in the hope of having a "tranquilizing" effect on the overoptimistic enthusiasm with which every new psychiatric therapy is likely to be greeted, and to which neither the dynamicist nor the pharmacophile is immune. As a research man himself Bailey would be the last one to condemn experimental methods of treatment. Many of our most competent investigators are carrying on just that kind of work. Without claiming cures they hold that relief from distressing symptoms and cutting short attacks of illness, whether for short or long periods, are immediate therapeutic goals in themselves. Such research work in clinical and laboratory settings must and will go on. In fact, while not disputing the value of psychotherapy, it is the kind of research that goes on in the chemical laboratory that Bailey specifically urges<sup>2</sup> as the most likely means of finding the ultimate causes of mental illness, thus opening the way to rational therapy. He sees evidences that psychiatry is turning more confidently in that direction.

<sup>1</sup> Some of the questions Freud raises as to the validity of his own hypotheses, and to which Bailey refers critically, might be turned to Freud's credit as evidence of healthy scientific doubt.

<sup>2</sup> He shares the view of Pierre Janet who stated that he expected the problem of schizophrenia to be solved by the chemist.

#### CORE OF MEDICINE

I have learned at least three principles since I have been in Paris [attending the clinics of Charles Pierre Alexandre Louis]: not to take authority when I can have facts; not to guess when I can know; not to think a man must take physic because he is sick.

—OLIVER WENDELL HOLMES

## NEWS AND NOTES

**NATIONAL FOUNDATION FOR INFANTILE PARALYSIS FELLOWSHIPS.**—The National Foundation for Infantile Paralysis has announced fellowships ranging from \$3,600 to \$6,000 a year for training of psychiatrists interested in the emotional problems of the physically disabled. These grants are offered only to physicians licensed to practice in the U. S. who have had 2 years of graduate training in psychiatry acceptable to the American Board of Psychiatry and Neurology.

Study is to be undertaken in a facility where medical and medical associate personnel are engaged in a program of comprehensive rehabilitation. The facility should be affiliated with a department of psychiatry approved by the American Medical Association and the American Board of Psychiatry and Neurology for residency training. The department of psychiatry is to be responsible for supervision of the Fellow.

Applications must be in by March 1 to be considered in May of that year; in by September 1 to be considered in November, and by December 1 for consideration the following February. Further information may be obtained from the Division of Professional Education, The National Foundation for Infantile Paralysis, 120 Broadway, New York 5, N. Y.

**NATIONAL ASSOCIATION FOR MUSIC THERAPY.**—A pamphlet, *Uses of Music in Institutions*, has been published by this Association reporting on a survey of the uses of music in institutions in the United States and Canada. This report covers mental hospitals, tuberculosis hospitals, hospitals for the crippled and mentally retarded, as well as general hospitals, and discusses the various ways in which music therapy programs can be organized in these different institutions. Copies may be obtained from the National Association for Music Therapy, Inc., c/o Mrs. M. F. Thompson, Essex County Overbrook Hospital, Cedar Grove, N. J.

**PSYCHIATRIC RESEARCH REPORT.**—Psychiatric Research Report No. 5, published by The American Psychiatric Association, is now available and contains the papers presented at the APA Mid-Atlantic Regional Research Conference, Washington, D. C., last March. The title of this report is "Research Techniques in Schizophrenia"; the main contributors are John C. Lilly, Jerome K. Myers, Morton Kramer, Barbara J. Betz, John C. Whitehorn, and Ogden R. Lindsley.

Copies of the report may be obtained from Psychiatric Research Reports, American Psychiatric Association, 1785 Massachusetts Avenue, N. W., Washington 6, D. C., for \$2.00 each.

**AMERICAN GROUP PSYCHOTHERAPY ASSOCIATION.**—The first annual Training Institute of the American Group Psychotherapy Association will be held on Wednesday, January 9, 1957, at the Henry Hudson Hotel, 353 W. 57th Street, New York City. This will be a 1-day, 3-session Institute with a group of the outstanding therapists of the country as instructors.

The Institute will be open to A.G.P.A. members, psychologists, and social workers who meet the minimum requirements for A.G.P.A. associate membership. The fee will be \$15.00 for members and \$20.00 for non-members. This includes registration, tuition fees, and dinner in the evening.

For further information write Director of Training Institute, American Group Psychotherapy Association, Room 300, 345 East 46th Street, New York 17, N. Y.

The 14th Annual Conference of the A.G.P.A., January 10-12, 1957, at the Henry Hudson Hotel, New York City, is open to members and nonmembers of the A.G.P.A.

**AMERICAN PUBLIC HEALTH ASSOCIATION 84TH ANNUAL MEETING.**—Reports on developments in the mental health field as related to public health will be presented on the

84th annual meeting program of the Association, in Atlantic City, N. J., November 12-16, 1956. Many eminent authorities in the field of mental health will be represented on the agenda of more than 400 scientific papers which deal with such other public health topics as community health organizations, health education, medical and dental care, health insurance and rehabilitation, and accident prevention.

The American Public Health Association, 1790 Broadway, New York, is the largest professional organization of public health workers in the world. Its president is Dr. Ira V. Hiscock, chairman of the Department of Public Health, Yale University.

**DR. MALZBERG TO MAKE DEMOGRAPHIC STUDY OF MENTAL DISEASE.**—Benjamin Malzberg, Ph. D., statistician in the New York State service for the past 33 years, has received a grant from the National Institute of Mental Health of \$188,607.00 for a 5-year study of demographic and related aspects of mental disease. The study will be based on department figures and depict the trends of mental disease in New York State, and their correlation with significant social factors.

**COL. GLASS HONORED.**—Upon his departure from Walter Reed Army Medical Center, Washington, D. C., Col. Albert J. Glass, chief of the department of neuropsychiatry, Walter Reed Army Hospital, was recently awarded a certificate of achievement in recognition of the quality of his services which "have been of the highest order, characteristic of the best traditions of the Army Medical Service." Col. Glass has been newly assigned to the Army Surgeon General's Office.

**ASSOCIATION FOR RESEARCH IN NERVOUS AND MENTAL DISEASE.**—The annual meeting of this Association will be held December 7-8, 1956, at the Hotel Roosevelt, New York City. The subject of the meeting will be "The Brain and Human Behavior."

**DR. HENNE NEW DIRECTOR OF NEWARK STATE SCHOOL.**—Dr. Frank R. Henne has been appointed director of Newark State School to succeed Dr. Isaac N. Wolfson, recently appointed senior director of Letchworth Village.

Dr. Henne has served for the past 8 years as assistant director of Harlem Valley State Hospital at Wingdale, N. Y. He began his state service in 1936 at Marcy State Hospital, advancing to senior assistant physician in 1941.

Dr. Henne is a graduate of the University of Toronto Faculty of Medicine. A diplomate of The American Board of Psychiatry and Neurology, he was recently certified by the A.P.A. as a qualified mental hospital administrator.

**DR. BELLAK HONORED.**—Dr. Leopold Bellak was chosen president-elect of the Society for Projective Techniques and Rorschach Institute at the Society's annual meeting in Chicago, August 31, 1956. This is the first time an M. D. has been chosen president of this Society.

**THE THOMAS A. C. RENNIE OBITUARY.**—In the biographical sketch and tribute to Dr. Rennie which appeared in the September issue of the Journal (page 287), the name of Dr. Oskar Diethelm, who kindly prepared this memorial notice, was inadvertently omitted. We tender our apologies to Dr. Diethelm.

#### DIAGNOSIS OF CATATONIA

Die Katatonie ist eine Gehirnkrankheit mit cyclisch wechselndem Verlauf, bei der die psychischen Symptome der Reihe nach das Bild der Melancholie, der Manie, der Stuporosenz, der Verwirrtheit und schliesslich des Blödsinns darbieten, von welchen psychischen Gesammtbildern aber eins, oder mehrere fehlen können, und bei der neben den psychischen Symptomen Vorgänge in dem motorischen Nervensystem mit dem allgemeinen Charakter des Krampfes als wesentliche Symptome erscheinen.

—KARL KAHLBAUM (1873)  
(*Die Katatonie oder das Spannungssirresein*)

## BOOK REVIEWS

**LE TEST DE RORSCHACH ET LA PERSONNALITÉ ÉPILEPTIQUE.** By *J. Delay, P. Pichot, T. Lemperière, and J. Perse.* (Paris: Presses Universitaires de France, 1955.)

This monograph is a model of clarity, organization, and constructive critical appraisal. The task the authors set themselves is to evaluate the problem of epilepsy with respect to 2 issues: (1) is there an "epileptic" personality?, and (2) what are the characteristics which might be so designated? The Rorschach test is used because of its potential value in rigorously answering both these questions; and there is a background for hypotheses in the large number of studies of epileptics which use the Rorschach as their major personality test. The volume is divided into 3 main sections. The first, "The Epileptic Personality," is a relatively brief survey of clinical observations on, and theories concerning epilepsy. The systematic approach of the authors is beautifully manifested in this section for they succeed in bringing order into the diversity of views on the nature of epilepsy and the source of epileptic personality traits. Fullest consideration is given to modern theories. The authors grossly divide these into the theories which regard the epileptic personality as innate (genetic and constitutional theories) and those which treat the epileptic personality as secondary or acquired. The latter theories are further subdivided into those which explain the personality characteristics of epileptics as somatically determined and those which adopt a psychological determinism. From the most general features to the most specific, they continue to subdivide these categories and then deal with each point of view in more detail. In view of the conflicting ideas, each partially supported by empirical findings, they reconsider the problem in the light of studies with the Rorschach test and pursue their own researches on it.

The second section, "This History of Rorschach's Test in Epilepsy," is a thorough evaluation of the available literature. Each study is presented with a summary of results, data on the nature of the sample and the research design, and a critical appraisal. This is a difficult task for presentation but the authors succeed in making it readable and highly informative. Some familiarity with Rorschach scoring is essential for an understanding of this section since the findings are given in terms of Rorschach scores and ratios. This section concludes with a summary review of the literature and an attempt to integrate the findings of the many studies—another excellent model of clarity and research acumen. The conclusions from their closely reasoned summary argument warrant paraphrasing. They are as follows: (1) epileptics, including those of average or superior intelligence, differ from normals (primarily with respect to "lowering of creative power . . . and a poverty of inner life," "emotional instability,

an egocentricity and impulsive approach to human problems."); (2) the epileptic personality does not seem to be simply a reaction to the social limitations of the illness; (3) epileptics show Rorschach signs characteristic of organic brain damage (even in cases with no demonstrable brain damage) with neurotic signs superimposed; (4) studies on constitutional and hereditary factors are inconclusive; (5) there is a relationship between the type of epilepsy and the nature both of the intellectual impairment and of personality abnormalities; (6) dementia in epileptics is not a sufficient explanation of the personality change that occurs; (7) two general polar types of personality emerge: (a) those with a general retention of intellectual functioning and with difficulties in social adaptation of an impulsive, egocentric type; and (b) those with a predominance of intellectual deficit with constricted but relatively satisfactory social adjustment.

Following this review of the literature is the third and last section, "The Rorschach Test with Fifty Epileptics." All 50 patients studied were ambulatory; the diagnosis of epilepsy was confirmed both clinically and by the EEG. Many variables are considered in their study: age, sex, date of onset, duration of illness, intellectual level, etiology and localization, behavior difficulties. Likewise, with respect to the Rorschach, those factors which proved most discriminating from among the many studies considered, were selected for further investigation. The approach they use is one of determining (1) the diagnostic value of each Rorschach sign; (2) the interrelationships among the signs; (3) the relationship between each sign and clinical characteristics of the patient group. The clearest and broadest finding, in part supported by the available literature, is the distinction between 2 subcategories of epileptics: (1) those with few Rorschach responses ( $R < 15$ ), rigid or constricted adjustment profiles (Rorschach's *erlebnistyp*), low color response score, and the absence of Mme. Minkowska's sign "liens" (an expanded conception of the movement response, emphasizing the language of the total response, particularly its "adhesive" quality, e.g., leaning, attached, seated; or its "explosive" quality, e.g., tearing off, burning, in an aggressive mood); and (2) the epileptics with 15 or more Rorschach responses, extratensive, outgoing adjustment, high color response score, and the presence of Minkowska's sign "liens." Contrary to other studies, they find no relationship between either of these 2 epileptic types and intelligence; and they find only a slight negative relationship between the presence of Piotrowski's organic signs in the Rorschach and intelligence. They obtain a strong relationship between the sign "liens" and the presence of character difficulties, the latter clinically determined. They also find relationships between the organic signs in the Rorschach and the

localization of brain damage, but no discriminating relationship to the etiology of the epilepsy.

This volume warrants close consideration by people working in the field of epilepsy and certainly by all those concerned with psychodynamic issues in epileptics. Beyond that, the book has more general interest. While their research design follows a fairly simple model, it is systematic and can serve as an example for the investigation of other clinical problems. However, both their results and the heuristic value of their original study are limited by their design; they do not choose to explore new issues, to cut across traditional categories, to analyze their data from many different vantage points. Within its limits, however, it is well conceived and well executed. The value of the Rorschach test in studying epilepsy and discriminating among epileptics on the basis of a number of variables is unambiguously demonstrated and gives further evidence, if such be needed, of the unique contribution this instrument offers to psychiatry and psychology.

MARC FRIED, PH. D.,  
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**LEHRBUCH DER ALLGEMEINEN PSYCHOPATHOLOGIE: GRUNDBEGRIFFE UND KLINIK.** By K. W. Bash, M. D.; preface by Professor H. Binder, M. D., and a contribution by H. Landolt, M. D. (Stuttgart: Georg Thieme Verlag, 1955. \$7.00.)

"Psychopathology is the mediator between psychology of the normal and psychiatry," the author states, and this textbook of general psychopathology was written in an endeavor to bring, for their mutual advantage, psychology and psychiatry again closer together. Recent results of psychiatric and psychological research have been integrated and comprehensively presented. They are used as a basis for new theoretical concepts, especially with regard to consciousness, *Bewusstheit* (awareness) and *Unbewusstheit* (unawareness), delusions and hallucinations, depersonalization, and other psychological concepts such as *Vitalantrieb* (life force) or libido.

The psychopathological phenomena are analyzed on the basis of the "Gestalt and Field" theory and from the viewpoint of depth psychology. Original is the author's organization of the material on the basis of structured psychological basic principles. The book is divided into 5 parts: Part I: The psychopathology of unstructured basic psychic phenomena is concerned with defining these concepts and demonstrating their polar characteristics. Part II presents a brief introduction into the psychological concepts of "Gestalt and Field," abstract and concrete thinking and behavior. Part III deals with the psychopathology of basic psychic processes, such as feeling, sensation, intuition, and cognition, which leads to new concepts about delusions and hallucinations. Part IV describes the more highly structured psychic functions: memory and falsifications of memory; disturbances of orientation, of the ego, perception and apperception, intelligence and personality; drives and instinct and their patho-

logical disturbances, which leads to Part V, in which clinical syndromes and their relationship to each other are discussed (the general adaptation syndrome of Selye, the syndrome of Gestalt disintegration, the acute exogenous reaction type, the organic psychosyndrome, the syndrome resulting from localized brain damage).

Summaries after each chapter and many well-chosen case reports illustrate the text material and facilitate the reading and understanding of the often problematical concepts. Diagrams, an index of examples of psychopathological phenomena, and a subject and author index are welcome additions. Literature references, wherever needed, are given in the text.

This book will be of great value to the student as an up-to-date introduction into the field of general psychopathology, and stimulate the more experienced to further research. Its use, however, is limited to those who have a full command of the German language. Unfortunately, even they will be confused by the author's frequent use of words subject to his own definition, as well as by his uninhibited coining of new ones.

MAX RINKEL, M. D.,  
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**PHRENOLOGY, FAD AND SCIENCE.** John D. Davies. (New Haven: Yale University Press, 1955. \$3.75.)

In the 1820's there had washed up on the western shores of the Atlantic a wave of interest in a new and exciting system of brain and mind study emanating like two later systems—mesmerism and psychoanalysis—from Vienna. The inventor of the new doctrine was Franz Joseph Gall (1758-1828), who on the assumption that character traits were localized in regions ("organs," 37 of them) of the brain concluded that these organs in turn affected the contour of the skull, the physical geography of which thus revealed the nature of the individual in mental health or disease.

Gall had begun lecturing on his theories in Vienna in the late 1790's, but in 1802 the Austrian government forbade these lectures as subversive of morals and religion. Meantime Gall had acquired a disciple, Johann Gasper Spurzheim (1776-1832), and leaving Vienna the two set out on their travels, settling, as Mesmer later did, in Paris. Spurzheim at first collaborated with his teacher and "played Brill to Gall's Freud," as Davies neatly puts it. Later he struck out on his own and became Jung to Gall's Freud, if we may so continue the figure. It was Spurzheim who coined the word phrenology which Gall refused to adopt. Spurzheim conducted missionary work in Britain with marked success. The *Edinburgh Review* (1815) however was not taken in. "The writings of Drs. Gall and Spurzheim have not added one fact to the stock of our knowledge . . . a mixture of gross errors, extravagant absurdities, downright misstatements . . . such is the trash, the despicable trumpery, which two men, calling themselves scientific enquirers, have the impudence gravely to present to the physiolo-

gists of the 19th century, as specimens of reasoning and induction." Nevertheless the Edinburgh barrister, George Combe, was converted to phrenology on a rebound from his earlier Calvinism, and became a powerful influence in spreading the new gospel.

But to come back to America, the land where mystics and metaphysicians, parapsychologists and messiahs from Buncombe County flourish, here phrenology also had its palmy days, the author lists banker Nicholas Biddle of Philadelphia as probably the first American phrenologist. The number of eminent persons who gave adherence or serious consideration to the new teaching cautions not alone against too ready acceptance of theory unsupported by evidence but also against too ready ridicule of such assumptions in the past. It is needless to view the historic perspective clearly. Earlier students or promoters of phrenology included Dr. John C. Warren, professor of medicine at Harvard, Dr. John D. Wells, professor at Bowdoin—both these teachers lectured on phrenology to medical students—Dr. William Physick of Philadelphia who headed the Central Phrenological Society, Dr. John Bell, editor of the *Philadelphia Journal of the Medical and Physical Sciences*, Dr. Charles Caldwell, "the American Spurzheim," who crusaded west and east converting many and forming new societies, and Dr. Benjamin Silliman of Yale.

In 1832 Spurzheim began a triumphal tour in the United States; it was cut short by his death after three months. He had been received with honors by both Yale and Harvard. The Boston Medical Society marched in a body at his funeral.

After the death of Spurzheim, George Combe was the leading phrenologist in the world. Emerson had said of his phrenological text, *The Constitution of Man*, that it was "the best Sermon I have read for some time." Combe took over where Spurzheim's barnstorming had left off—158 two-hour lectures in 18 months throughout the East. His large audiences "were from the intellectual elite of the country." Some 40 to 50 societies had sprung up to promote the new psychology and a spate of phrenological literature flooded the country. The craze was taking on the character of a psychic epidemic.

It was not, however, without its critics like any other cultish doctrine based on a mickle of plausible observations and a muckle of zeal and enthusiasm. The most famous single attack was launched by Dr. Thomas Sewall, professor of anatomy at the Columbian College, Washington, D. C. (1828).

At Amherst College (1833) a student debate was staged on phrenology. Henry Ward Beecher took the negative side and won gloriously, but immediately thereafter he announced his unqualified acceptance of the new religion to which he remained a life-long devotee.

During the 1840's phrenology spread like a religious revival through the Eastern cities of the United States, mainly through the operations of the Fowler brothers (to whom a whole chapter is devoted), and gradually moved westward. It became a vulgar tent-show where the gullible paid

for guidebooks to all questions of health, eugenics, matrimony, religion, and self-enhancement generally.

And it was not alone the less intelligent who had their heads read; myriads of the most prominent citizens of their day lent their prestige to the new movement—among these: James A. Garfield, John Brown, G. Stanley Hall, Walt Whitman, Edgar Allan Poe, Joseph Smith, Clara Barton, Charles Sumner; also, across the Atlantic, Queen Victoria and Prince Albert, Bismarck, Karl Marx, Baudelaire, Balzac, George Eliot. Henry George was proficient enough to analyse his own head. As might be expected, violent controversy and opposition accompanied the missionary of the phrenologists throughout their crusades. John Quincy Adams "could not see how two phrenologists could look each other in the face without bursting into laughter. Oliver Wendell Holmes . . . read a sarcastic lecture to the 'boarders' at his Breakfast Table on this 'pseudo science.'"

Thus far we have been dealing with the historical side of Mr. Davies' study. The next 100 pages discuss in separate chapters the part phrenology has played in education, insanity, penology, health, literature, phrenomagnetism, medicine and religion—a prodigious program but not beyond the ambition of this new revelation, "variously termed a social science, a universal philosophy, a guide to reform life itself."

In education, Horace Mann and Dr. Samuel Gridley Howe became devotees. Many of the mid-nineteenth century pioneers in psychiatry also accepted the new doctrine. These included Amaeriah Brigham, H. A. Buttolph, Samuel B. Woodward, Isaac Ray. A number of articles on phrenology appeared in the early numbers of the *American Journal of Insanity*. In his famed "The Medical Jurisprudence of Insanity" Isaac Ray frequently quoted Gall, Spurzheim, and the Combess.

The reader will have noticed that Davies devotes a chapter to "phrenomagnetism," and may have wondered what that is. It was the offspring of two pseudo-sciences. Mesmerism had reached the American shores during the 1830's and was soon competing with phrenology for customers. "Animal magnetism" became a popular commodity. Then, on discovery that magnetizers by "placing their hands upon the separate mental (phrenological) organs of their 'patients' during their 'magnetic sleep,' and thereby . . . inducing these faculties to exhibit their 'appropriate language' in a 'pure state,'" it became apparent that here was something the universal science could not ignore, could even use; so, after some tremors of conscience, she opened her arms and phrenology had a new department—phrenomagnetism.

During the 1840's phrenology, especially after incorporation of the show business of the magnetizers, began to lose favor with those of the medical profession who had earlier been sympathetic. It was true however that it was not a total loss. It had emphasized the importance of the study of brain structure and the localization of special functions and these leads were of value for both medicine and anthropology.

The clergy was fairly united in hostility to phrenology, as a system of materialism, but much of the pulpit invective, the author suggests, "maybe charged simply to the clergy's dislike of innovation and suspicion of science in all its various forms."

It is now a century and a half since phrenology was invented. It has been called a social science and a new religion and by other less complimentary terms. Henry Ward Beecher declared that his whole ministry was based upon it, and that a "practical knowledge of the human mind as is given by phrenology" is the best preparation for the Christian.

From first to last phrenology made a stir in the world for more than 100 years.

The *American Phrenological Journal* did not fold until 1911. In its vigor, however, as Davies puts it, "it had a one-generation career." Men of critical judgment, allured at first by its revolutionary claims, could still see, after discarding its head-readings and the other extravagances, that it had made its contribution to the scientific study of man, of brain function, of anthropology, of experimental psychology. Indeed Edwin G. Boring could write (1929), "It is almost correct to say that scientific psychology was born of phrenology, out of wedlock with science."

All these things Mr. Davies tells in his book.  
C. B. F.

**THE BRITISH ENCYCLOPEDIA OF MEDICAL PRACTICE: MEDICAL PROGRESS, 1955.** Edited by The Lord Horder. (London: Butterworth, 1955.)

This annual volume bringing up to date some of the important advances in the wide field of medicine is well known and follows the same form.

In these comparatively early days of antibiotics, the nature of infection, the meaning of immunity, the relation of host to parasite assumes importance of even greater dimensions than ever.

The first flash of excitement at the introduction of antibiotics suggested the possibility that bacteriology might even die out! Instead, a whole new set of problems has arisen in the study of the organisms which compete in our environment. This problem is present in all fields. In the critical surveys, of which there are 12, all systems of the body are reviewed in the newer aspects which have been discovered as to etiology and treatment. Blood diseases and their mechanism and treatment are reviewed as more settled opinions develop. In surgery, the thyroid gland and its tumors are of special interest. Breast carcinoma, thoracic surgery in general and cardiac and vascular lesions are dealt with. Progress in pulmonary tuberculosis is fully reviewed in all its aspects.

As everyone recognizes that anesthesia has made remarkable surgical procedures possible, a full re-

view of the strides made in the specialty is most welcome.

The survey on endocrinology brings the state of affairs in regard to the pituitary and the adrenal up to date. There is an interesting article on malignant disease mostly concerning etiology. The urological review contains nothing especially new, but among the items of interest are some accounts of experiences with carcinoma of the prostate and the problems of urinary infection.

Recent developments in pharmacology and therapeutics mostly concern antibiotics, anticoagulants, and oral diuretics. The abstracts from "the literature" are useful, but perhaps not as numerous as in previous years.

TREVOR OWEN, M. D.,  
Toronto, Canada.

**AN INTRODUCTION TO PSYCHIATRY.** By Max Valentine, M. D., D. P. M. Baltimore: 1955. \$3.75.)

In his preface the author quotes the Duke of Wellington "Publish and be damned." Recognizing this possibility he states that his reason for writing yet another book is an attempt to present psychiatry for the medical reader in such a way that it will be consistent with his training in applied biological science. The over-all reaction to this book is difficult to pin down. One gets the impression of a very fine motor which is not tuned to its optimum performance; goes along, purring smoothly, and then sputters and jerks. It seemed to us a rather heroic undertaking, starting out with a brief history of psychiatry then going through the various mental illnesses, dwelling on electroencephalography and clinical psychology and winding up with forensic psychiatry. The latter is obviously limited to the British Isles. There is included an appendix with verbatim interviews to demonstrate the illnesses previously alluded to. The author's groupings and classifications of the various syndromes do not follow most systems. Still, there cannot be too great a quarrel with his personal opinion. He states "The volume is divided into sections rather than chapters and decimal sub-division is used in place of page-numbering." If there is an advantage in this arrangement it escaped the reviewer. The book is nicely bound and the print is pleasant to read. It is small, being some  $7\frac{1}{2}$  by  $4\frac{1}{2}$  inches and about half an inch thick, and fits rather well into a coat pocket. Dr. Valentine has definitely made a sincere effort. Whether he has succeeded or not in his original aim will probably be dependent upon one's training in biological science. However, this little book could well serve as "An Introduction to Psychiatry."

WILLIAM K. KELLER, M. D.,  
Louisville, Ky.



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1. Yohe, C.D.: in *Chlorpromazine and Mental Health*, Philadelphia, Lea & Febiger, 1955.

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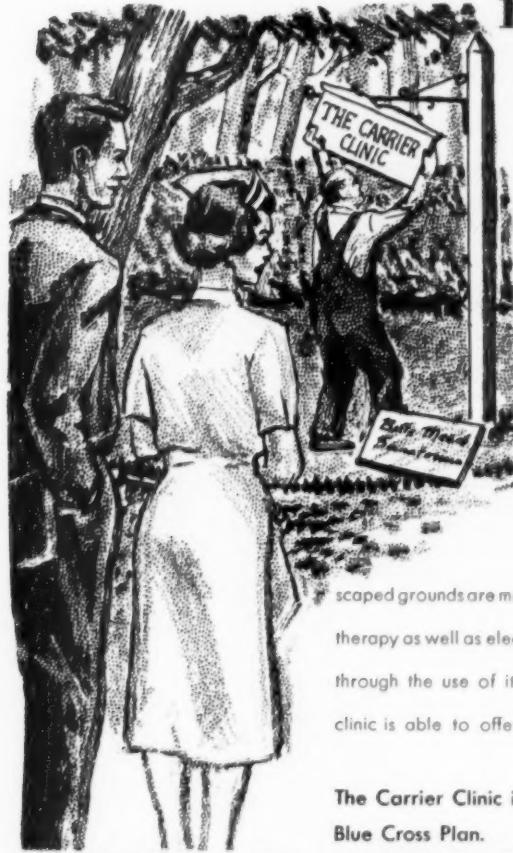
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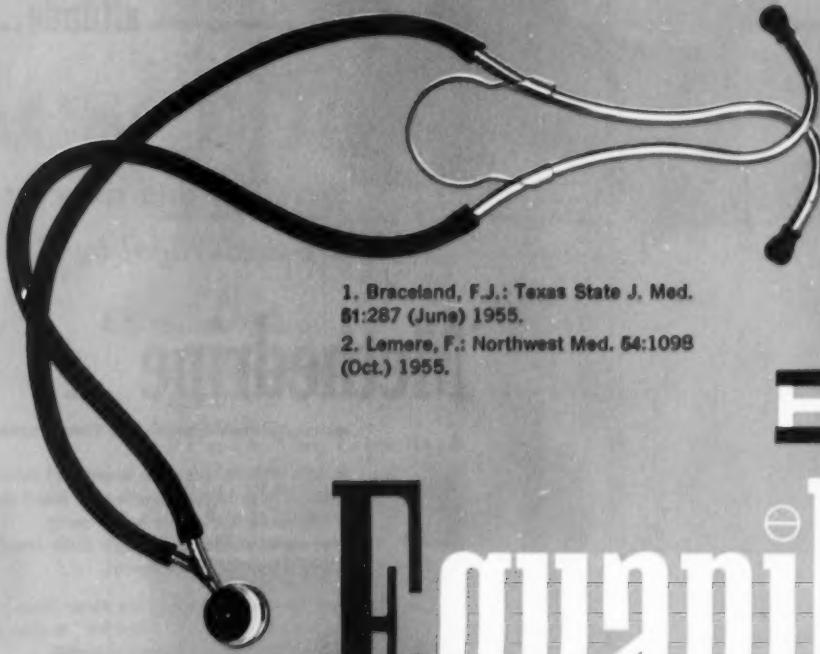
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1. Braceland, F.J.: Texas State J. Med. 51:287 (June) 1955.
2. Lemere, F.: Northwest Med. 54:1098 (Oct.) 1955.

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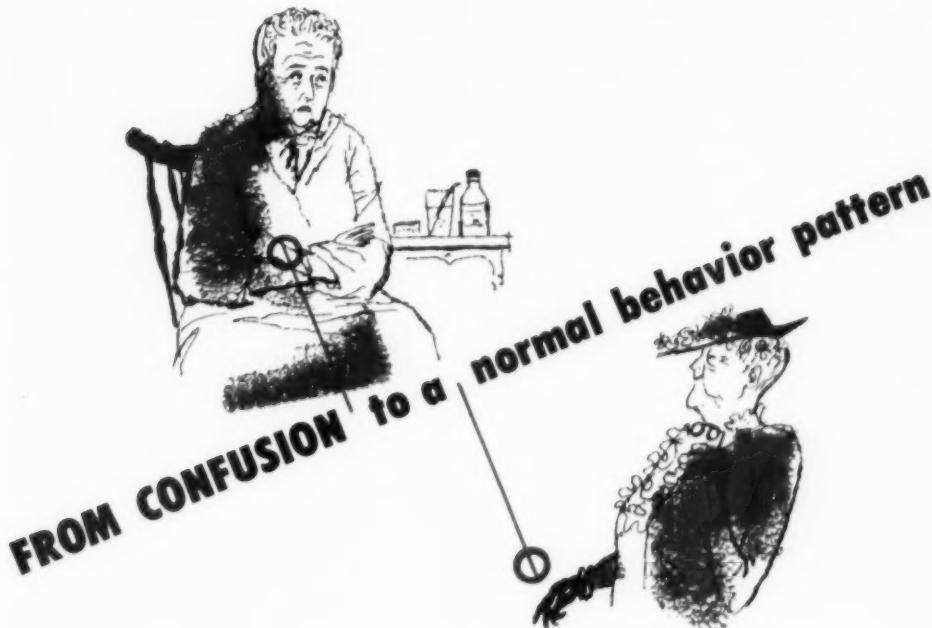
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1. Levy, S. J.A.M.A. 153:1260, 1953.

2. Thompson, Lloyd & Proctor, Rich.

N.C. State, Dec. '54

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1. Hollister, L. E., Krieger, G. E., Krugel, A., and Roberts, R. H.: Ann. New York Acad. Sc. 61:92 (April 15) 1955.

2. Hoffman, J. L., and Konchegul, L.: Ann. New York Acad. Sc. 61:144 (April 15) 1955. 3. Kline, N. S., and Stanley, A. M.: Ann. New York Acad. Sc. 61:36 (April 15) 1955.

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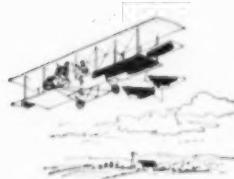
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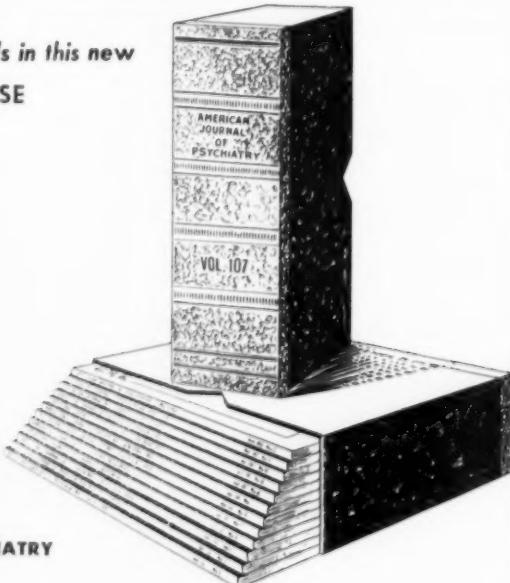
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